PROCEEDINGS

OF THE

ROYAL SOCIETY OF LONDON.

From June 1 *to December* 14, 1893.

VOL. LIV.

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PROCEEDINGS

OF

THE ROYAL SOCIETY.

June 1, 1893.

The Annual Meeting for the Election of Fellows was held this day.

The LORD KELVIN, D.C.L., LL.D., President, in the Chair.

The Statutes relating to the election of Fellows having been read, Professor Lankester and Mr. Mond were, with the consent of the Society, nominated Scrutators to assist the Secretaries in examining the lists.

The votes of the Fellows present were then collected, and the following candidates were declared duly elected into the Society :—

Burnside, Professor William, M.A.
Dunstan, Professor Wyndham R., M.A.
Ellis, William, F.R.A.S.
Ewart, Professor J. Cossar, M.D.
Gairdner, Professor William Tennant, M.D.
Hobson, Ernest William, D.Sc.
Howorth, Sir Henry Hoyle, K.C.I.E.
Newton, Edwin Tulley, F.G.S.
Sherrington, Charles Scott, M.D.
Stirling, Edward C., M.D.
Thornycroft, John Isaac, M.Inst. C.E.
Trail, Professor James William Helenus, M.D.
Wallace, Alfred Russel, LL.D.
Worthington, Professor Arthur Mason, M.A.
Young, Professor Sydney, D.Sc.

Thanks were given to the Scrutators.

June 1, 1893.

The LORD KELVIN, D.C.L., LL.D., President, in the Chair.

Professor Alexander Pedler (elected 1892) was admitted into the Society.

A List of the Presents received was laid on the table, and thanks ordered for them.

The President gave notice that at the next meeting of the Society he would propose the Duke of York for election as a Fellow of the Society by immediate ballot, to which, as a Prince of the Blood Royal, His Royal Highness was entitled.

The following Papers were read:—

I. "On the Colours of Sky Light, Sun Light, Cloud Light, and Candle Light." By Captain W. DE W. ABNEY, C.B., D.C.L., F.R.S., P.R.A.S. Received May 9, 1893.

The author has made several comparisons of the above lights throughout the different parts of their spectra, and has been able to verify their correctness by means of templates rotating in the spectrum of electric light, as described in Part II, "Colour Photometry," 'Phil. Trans.,' 1889. It seemed, however, that it would be useful if the colours of these lights could be expressed in single wave-lengths, together with the amount of added standard white light, the latter being expressed in terms of the luminosity of the dominant colour, in accordance with the method brought before the Royal Society in 'Proc. Roy. Soc.,' 1891.

When measuring light from the sky, a beam from the zenith or other desired part was reflected through a blackened tube into a darkened room in which the colour patch apparatus ("Colour Photometry," Abney and Festing, 1886) was placed, and the image of the end of the tube was focussed on to the front surface of a cube, the front surface of which was coated with zinc white, its background being black velvet. The patch of colour from the apparatus was also thrown on the cube. A rod placed in the paths of the two beams enabled the sky light and the spectrum colour to be examined side by side. The slit in the spectrum was an adjustable one, so that any intensity of colour within limits would fall on the cube. A beam of

white light reflected from the first surface of the first prism was again reflected from the surface of a thin prism on to the cube, a rod placed in its path cast a shadow on that part illuminated by the sky light, and by suitable adjustment the boundaries of the two shadows were caused to exactly coincide. The colour was thus diluted with white light, and rotating sectors, described in other papers, being placed in the path of the white beam, enabled the dilution to be regulated.

Sky Light.—On June 27, 1892, at 2.30 P.M., the sky was a good blue, but not a dark blue, and perhaps rather milky. The slit was moved into the part of the spectrum which appeared to be near the dominant colour. The colour was diluted to approximately the required amount. The slit was shifted and the dilution altered until the two colours made a perfect match. It was found that on the standard scale of the spectrum the dominant colour was represented by 28·6, which is λ 4800. The mean value of the sector aperture was 32°, and recollecting that the sectors are double sectors the comparison has to be made with 180°. The next operation was to compare the luminosity of the whole beam of white light with that of the colour. The sectors still remained in the white; the sky light was cut off, and the rod altered till the colour and the white were alongside each other with the boundaries of the shadows touching. The luminosities of the two were compared, and it was found that the aperture of the sector was 14°. As it required 32° of white to make the dilution of the colour, it follows that 32/14, or 2·286, parts of white were required to dilute 1 part of the blue. This may be expressed thus—

$$\text{Sky light} = \lambda\,4800 + 2{\cdot}3\,\text{W}.$$

On July 4, 1892, at mid-day, the same procedure was adopted, and the dominant wave-length was again λ 4800. In this case the amount of added white was thus—

$$\text{Sky light} = \lambda\,4800 + 3{\cdot}1\,\text{W};$$

in other words, the sky was more milky.

At 4 P.M. on the same day the sky to the east, and about 30° above the horizon, was evidently slightly greener, and it was found that the colour agreed with scale No. 29·6, or λ 4834, and that it required 3 parts of white to be mixed with it.

$$\text{Sky light} = \lambda\,4834 + 3\,\text{W}.$$

On other days, with the light of the portion of the sky near the zone of maximum polarisation the dominant wave-length was found to be between these two limits, and was never found bluer, and the smallest admixture of white light was found to be 1·9.

From these measures it may be concluded that the dominant colour of a blue sky is λ 4800.

Amongst artists it is not uncommon to employ cobalt to render this colour, and in many instances this is mixed with Chinese white.

The dominant colour of cobalt was found to be at scale No. 29, or λ 4812, when illuminated by ordinary day light, whence it seems that, as far as colour is concerned, it is singularly fit for the purpose.

Sun light was compared in the same manner, but the beam was reflected from the surface of a prism into a dark room, and again diminished in intensity by placing in its path rotating sectors with very narrow apertures.

Near mid-day on July 8 the sun was very clear, the sky being free from clouds, and a strongish wind blowing from the west. Two separate sets of measures were made with an interval of an hour between each. It was found that the dominant colour was λ 4885 in both cases, and in the first set it was diluted with 5·45 of white, and in the other with 5·14 of white. This indicates that sun light contains slightly more green-blue rays than the light emitted from the crater of the positive pole of the electric light. This agrees with the spectrum measures made in "Colour Photometry."

Cloud light was next matched on days in which the sky was overcast. A comparison of the general light of the zenith was all that was attempted, and near mid-day.

It was found that it required 1 part of λ 4864 diluted with 5·5 parts of white to make a match. It will be seen that the dominant colour of cloud light lies between that of the sky and of the Sun, as might be expected, and is decidedly whiter than the sky, as might also be anticipated.

Various comparisons of sunset colours have been made, and found to range from λ 6300 up to λ 4800; in some cases it was necessary to match by means of complementary colours.

The light from a paraffin candle it was found could be very closely matched with D sodium light. The equation may be expressed as follows :—

$$\text{Candle light} = \lambda\,5880 + 0{\cdot}4\,\mathrm{W}.$$

The amount of added white varied from 0·1 to 0·5, and it is in this part of the spectrum that a large number of separate observations are required in order to get a good and fairly trustworthy mean.

II. "Flame Spectra at High Temperatures. Part I. Oxyhydrogen Blowpipe Spectra." By W. N. HARTLEY, F.R.S.

Received May 10, 1893.

(Abstract.)

Brewster, in 1842, first examined the spectra of salts with a flame of oxygen and coal-gas ('Proc. Roy. Soc. Edin.,' vol. 6, p. 145).

Professor Norman Lockyer has given us maps of twenty-two metallic spectra at the temperature of the oxygen and coal-gas flame. The region observed lies between λ 7000 and 4000.

Preparatory to undertaking the study of spectroscopic phenomena connected with the Bessemer "blow" and the manufacture of steel generally, I have carefully observed the spectra of metals and metallic oxides obtained by submitting the substances to the oxyhydrogen flame.

Method of Investigation.—The method of obtaining spectra with flames at high temperatures is the following. Hydrogen proceeding from a large lead generator is burnt in a blowpipe with compressed oxygen. The blowpipe measures 3 in. in length by $\frac{3}{8}$ in. external diameter. The substances examined are supported in the flame on small plates of kyanite about 2 in. in length, $\frac{1}{20}$ in. in thickness, and $\frac{1}{4}$ in. in width. This mineral, which is found in masses in Co. Donegal, contains 96 per cent. of aluminium silicate, and is practically infusible. The spectra were all photographed with the instrument employed by me on former occasions for photographing ultraviolet spectra, illustrations of which were published in the 'Chem. Soc. Journ.,' vol. 41, p. 91, 1882. The dispersion of the instrument was that of one quartz prism of 60°.

Isochromatic plates developed with hydroquinone were largely used. Various dyes for sensitising and all kinds of developing substances were tried. The spectra were measured with an ivory scale divided into hundredths of an inch, and directly applied to the photographs, the division 20 on the scale being made to coincide with the yellow sodium line which appears in every photograph. It was found convenient to record the measurements on a gelatino-bromide paper print taken from an enlarged negative. Sometimes, for more careful and minute reference, it was found convenient to make an enlargement of the spectrum with the scale in position, but accurate measurements cannot be made in this way. It is necessary to use a low magnifying power and cross wires in the eye-piece.

For the identification of lines already known nothing more complicated is required, but to measure new lines and bands it was considered desirable to make use of a micrometer and microscope, the

screw of the micrometer was cut with 100 threads to the inch, and the magnifying power generally used was 10 diameters.

Characters and Extent of the Spectra observed.—Just as in the ordinary use of the spectroscope we must be prepared to see the lines of sodium, and in hydrocarbon flames the bands of carbon, so in these spectra the sodium lines and the strongest lines belonging to the emission spectrum of water vapour are also always present.

In addition, the kyanite yields the red line of lithium, which is no inconvenience but a positive advantage, serving, as it does, to indicate where the spectra commence.

A large majority of the metals and their compounds all terminate somewhere about the strongest series of water vapour lines. Typical non-metallic spectra are sulphur, selenium, and tellurium; the first yields a continuous spectrum with a series of beautiful fluted bands, the second a series of fine bands, occurring at closer intervals, and the third is characterised by bands still closer together and near the more refrangible termination of which four lines occurring in Hartley and Adeney's spark spectrum of tellurium are visible. Increase in atomic mass causes shorter periods of recurrence of bands. In line spectra it is the reverse; increase in atomic mass causes greater periods in the recurrence of lines. Charcoal and carbon monoxide yield chiefly continuous spectra; the latter, however, exhibits some carbon lines. The hydrocarbons yield the well-known spectrum of carbon bands with also those attributed to cyanogen. Of metallic elements, nickel, chromium, and cobalt yield purely line spectra; antimony, bismuth, silver, tin, lead, and gold beautiful banded spectra (spectra of the 1st order) accompanied by some few lines. These spectra are finer than those of selenium and tellurium.

Iron and copper exhibit lines, and, less prominently, bands. Manganese has a beautiful series of bands and a group of three very closely adjacent lines. Aluminium gives a fine continuous spectrum with three lines, origin uncertain, zinc a continuous spectrum without lines, and cadmium a spectrum consisting of one single line only, λ 3260·2.

Of compounds, chromic trioxide yields a continuous spectrum with six lines belonging to the metal, copper oxide a fine band spectrum with two lines of the metal, magnesium sulphate gives a spectrum of magnesium oxide consisting of broad degraded bands composed of closely adjacent fine lines and one line belonging to the metal, λ 2852.

The sulphates of calcium, strontium, and barium give both bands of the oxides and lines of the elements. Phosphorus pentoxide yields a continuous spectrum with one peculiar line, seen also in the spectrum of arsenic.

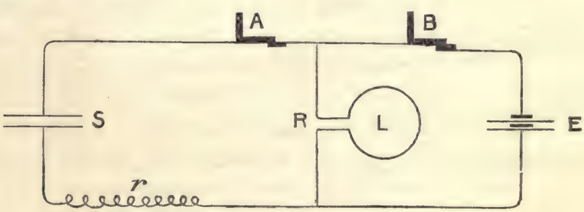
The chlorides of the alkalis give also lines of the elements with a more or less continuous spectrum, which, it is believed, is due to

the metal in each case. Lithium chloride gives no continuous spectrum.

The Volatility of Metals.—One of the most interesting facts ascertained by this investigation is the volatility of all the metals examined, except platinum, and particularly the extraordinary volatility of manganese, and, to some extent, of the infusible metal iridium. Metal believed to be pure iridium is seen to have diminished after the flame has played upon it for about two hours.

III. "On the Flow in Electric Circuits of Measurable Inductance and Capacity; and on the Dissipation of Energy in such Circuits." By ALFRED W. PORTER, B.Sc., Demonstrator of Physics in University College, London. Communicated by Professor G. CAREY FOSTER, F.R.S. Received May 4, 1893.

The arrangement of the apparatus in the experiments here described was as follows :—



L is a coil of self-inductance L (= 0·42 henry) and of resistance R (= 28 ohms).

S is a condenser of capacity S ($= 5 \times 10^{-6}$ farads), and in the same branch with it is an inductionless resistance, r, the value of which can be varied.

E is a battery which, when the circuits shown are complete, produces a current through L, and charges the condenser.

A and B are the two contact pieces of a pendulum interruptor. The two circuits can be broken at these places by the pendulum : the time interval between the two ruptures being regulated by the distance between the contact pieces.

One centimetre distance apart corresponds to 5·270 thousandths of a second, and, as this distance can be read easily (by a vernier attached) to a tenth of a millimetre, it is possible to measure intervals of

5 hundred-thousandths of a second.

The method of the experiments is as follows :—

A and B are initially closed; a steady current, x_0, flows in consequence through L, and the condenser is charged to a difference of potential Rx_0. The pendulum breaks contact first at B. This prevents further flow in the battery branch; the coil current is diverted into the condenser branch, and flows there until its energy is wholly dissipated or until its flow is intercepted by the rupture at A. The charge retained by the condenser is then measured by discharging it through a D'Arsonval galvanometer (not indicated on the diagram) which has been calibrated for ballistic use.

This series of operations is successively repeated for many values of the time interval.

It is thus possible to determine the charge of the condenser at any moment after the rupture of the battery branch. Some of the results obtained are shown in fig. 1 and fig. 2. The ordinates represent the charges in arbitrary units; the abscissæ give the time in thousandths of a second.

The data for the curves are as follows:—

		Value of r in ohms.	Inductance in henries.	Capacity in farads.
Fig. 1	Curve A....	10,000	0·42	5×10^{-6}
	Curve B....	3,100	,,	,,
	Curve C....	552	,,	,,
Fig. 2		0	,,	,,

Fig. 1, Curve A represents a merely leaking discharge;

Curve B represents the critical discharge that just fails to ever charge the condenser negatively;

Curve C represents the critical discharge that just fails to be oscillatory;

And the curve in fig. 2 represents a typical oscillatory discharge.

To find from theory what these curves should be, we must solve the equation

$$\left\{ L\frac{d^2}{dt^2} + \rho\frac{d}{dt} + \frac{1}{S} \right\} Q = 0,$$

where ρ is the dissipation constant, and Q is the charge at any instant. The constants of integration must be determined to suit the conditions that

$$Q_{t=0} = Q_0;$$

$$\left\{ -\frac{dQ}{dt} \right\}_{t=0} = x_0 = \frac{Q_0}{RS}.$$

The solution has one of two forms according as

$$\rho^2 \gtrless \frac{4L}{S}.$$

In the former case it becomes finally

$$Q = \frac{Q_0\epsilon^{-mt}}{2n}\left\{\left(m+n-\frac{1}{RS}\right)\epsilon^{nt}+\left(\frac{1}{RS}-m+n\right)\epsilon^{-nt}\right\},$$

where $$m = \frac{\rho}{2L}$$

and $$n = \sqrt{\left(m^2-\frac{1}{LS}\right)}.$$

In the second case let $p^2 = -n^2$, and the solution is

$$Q = Q_0\epsilon^{-mt}\sec\phi \,.\, \cos(pt+\phi),$$

where $$\tan\phi = \frac{\frac{1}{RS}-m}{p}.$$

Calculating and plotting the curve for the case in fig. 2 on the assumption that ρ is equal to the wire resistance in the circuit (28 ohms), the dotted curve in the same figure is obtained. The time periods of the two agree very well; but a marked difference is seen in the rate of shrinkage of the ordinates.

The explanation that offered itself is that the wire circuit is not the only seat of dissipation of energy, but that dissipation also takes place in the dielectric of the condenser. In accordance with this, it is possible to reproduce the experimental curve by increasing the value of ρ to 59·43 ohms. Points on the curve so determined are shown as solid dots in the figure. The agreement of the time periods is also improved by this increase in ρ, as can be seen from the following table:—

	Calculated from $\rho = 28$.	Calculated from $\rho = 59·43$.	Observed.
Time period in seconds....	0·009116	0·009154	0·009147

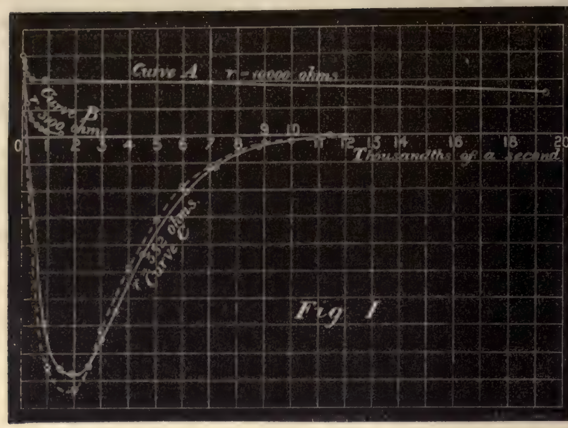
The experiment has also been repeated with soft iron rods inserted in the coil. These are rather longer than the coils, and their diameters are—

No. I	1·93 cm.
No. II	1·30 „
No. III	0·65 „

The other data were the same as for fig. 2. The curves obtained are shown in fig. 3, and are numbered I, II, III, according to the rod employed. Their chief characteristics are:—

α. A change in time period as the discharge progresses.

β. Rapid decrement.


Curve A r = 10000 ohms
Curve B r = 500 ohms
r = 352 ohms Curve C
0 1 2 3 4 5 6 7 8 9 10 11 12 13 14 16 18 20
Thousandths of a second
Fig I

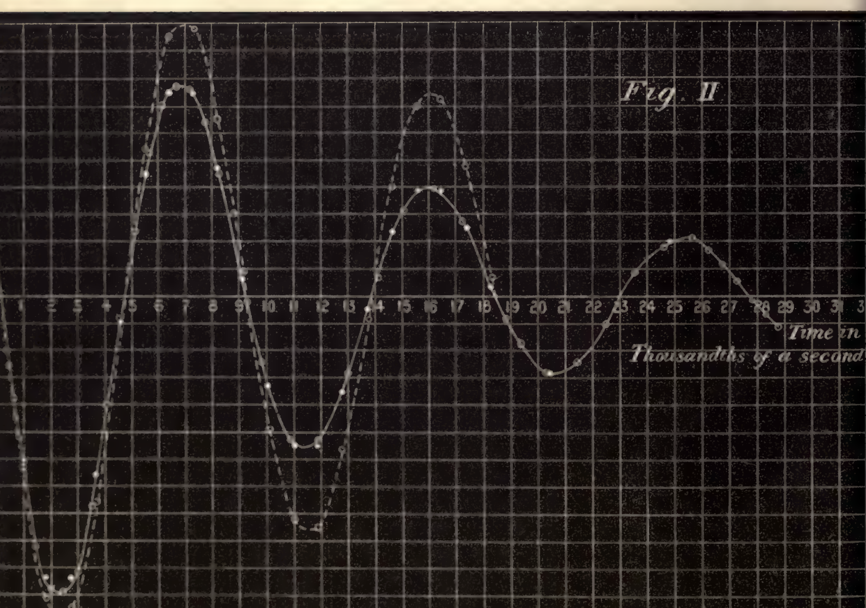


Fig II
1 2 3 4 5 6 7 8 9 10 11 12 13 14 15 16 17 18 19 20 21 22 23 24 25 26 27 28 29 30 31 32
Time in
Thousandths of a second

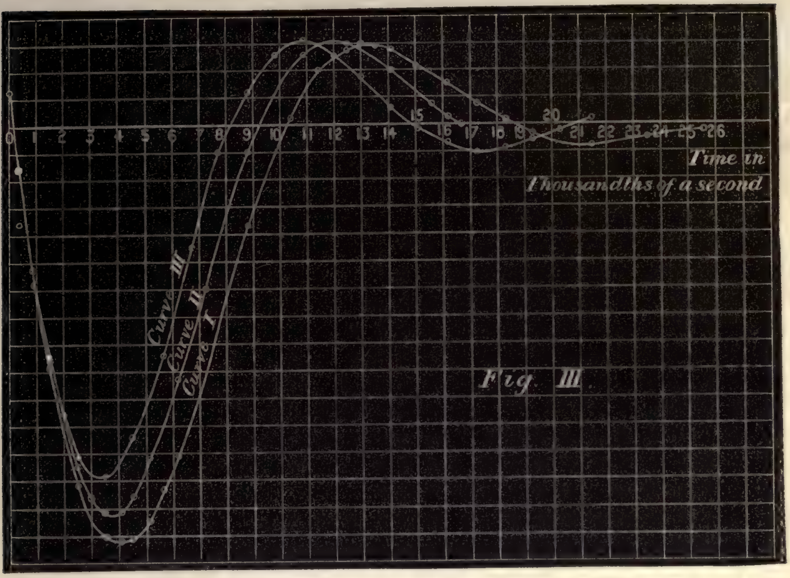


Fig. III

That this latter is only very partially due to eddy currents in the iron was shown by repeating the experiment with a brass rod of 2 cm. diameter inserted in the coil. The curve obtained is only slightly (though distinctly) different from that obtained without any core.

Experiments have also been begun on the decay of current in circuits containing iron and of negligible capacity. The coil possessing the inductance forms one arm of a Wheatstone's bridge. These experiments were commenced as far back as June, 1890; the experiments described above were commenced in January, 1891. Both series were suspended for want of a sufficiently precise interruptor. This has since been obtained, and satisfactory work has thus been made possible. The investigation is only in an early stage; but the fact that at least one other observer* is already working in the same field induces me to present this preliminary note in order to show the independence of our work.

* P. Janet. See 'Comptes Rendus,' vol. 115, Nos. 21 and 26; vol. 116, No. 8.

IV. "On the Motion under Gravity of Fluid Bubbles through Vertical Columns of Liquid of a different Density." By F. T. TROUTON. Communicated by Professor FITZGERALD, F.R.S. Received May 3, 1893.

The kind of motion herein referred to can be observed by means simply of a glass tube, closed at one end, and provided with a stopper. If the tube be filled with water to nearly the top, closed, and then placed upside down, the enclosed bubble of air while ascending to the top can be observed, and the speed of ascent ascertained between two measured marks.

By enclosing different volumes of air it was found that the speed depended on the length of the bubble. The relation connecting the volume of the bubble with its speed of ascent was experimentally investigated. The speed, as will be seen from experiments subsequently described, may be taken within limits as a periodic function of the volume of the bubble. Bubbles greater than a certain thing all have the same velocity.

Experiments have also been made with other liquids. By mixing two liquids, such as water and glycerine, a series of determinations of speed with liquids of gradually increasing viscosity can be made. In these experiments the size of the bubble was outside the periodic limit. Contrary to expectation it was found that as the viscosity of water was increased by adding glycerine, the velocity increased instead of diminished. With tubes of about 0·7 cm. in diameter, it is not until the viscosity of the solution used is about eight times that of water that the velocity comes to be less than that through pure water. From this state the *velocity* was found to be *inversely proportional to the viscosity* of the solution, other things the same.

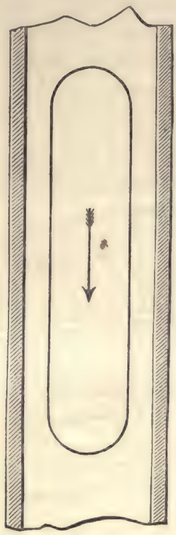
Instead of air the bubble may be of some liquid which does not too readily mix with that of which the column consists. In this way experiments were made to investigate the relation between the speed of ascent and the difference in density of the liquids, and also whether surface tension may have any influence.

The consideration of the subject may then conveniently be divided into two parts. The first part will deal with the dependence of the speed with which the bubble travels through the liquid column on the physical properties of the two fluids concerned in the phenomenon. The second part will refer to the connexion between the size or volume of the bubble and its speed.

PART I.

The physical properties involved in the phenomenon may, perhaps, be best studied by taking a particular case, say that of a bubble of chloroform falling through a column of glycerine contained in a glass tube. The appearance* is very much as shown in section in the figure.

FIG. 1.



In order that the bubble may descend, the liquid in front has to pass up the sides through the narrow annular space between the bubble and the tube. Were the diameter of the bubble known, the question would reduce itself to a case of viscous flow through an annular space—provided we neglected the ends and supposed the walls of the bubble to be rigid. The annular width will be seen to depend on the surface tension between the liquids, for should the tension become very great, say, the bubble must swell out, blocking up the tube. This tendency is in part counterbalanced by the excess in density of the bubble over that of the liquid column. The case is, so to speak, then that of viscous flow through an adjustable annular orifice.

The pressure per centimetre, or the pressure gradient driving up the liquid through this annular space, depends simply on the difference in density of the liquids.

* The length of the bubble was always several times its diameter, so as to get outside the stage where the velocity depends on the length of bubble.

The surface of the bubble moves by no means in a rigid manner, as can be seen by watching the movements of little particles of dust which may be present. The liquid of the bubble is seen in constant circulation flowing up the side with the current of glycerine, and returning down the centre of the bubble. Thus the viscosity of the liquid of which the bubble is composed must affect the velocity of its descent, but in what follows it has been neglected; this could be done, because the viscosity of the liquid of the column in most of the experiments was very great compared with that of the bubble.

Collecting the various things on which the velocity may depend, we have: 1, the pressure gradient, that is to say, the difference in density of the two liquids multiplied by the acceleration of gravity δg; 2, the viscosity of the liquids μ; 3, the surface tension between the liquids S; 4, the diameter of the tube D. It is difficult to see that anything else could come in to affect the rate of flow unless it be a slipping over the solid surface.

Thus we may put

$$V = f(\delta g \mu S D).$$

Assuming the function to have the form

$$V^{-1} = \Sigma A\,(\delta^x g^y \mu^z S^m D^n),*$$

we can obtain three equations from the considerations of dimensions to help determine the unknown exponents.

$$\begin{aligned} &\text{From length,}\dagger & -1 &= -3x+y-z+n, \\ &\text{From mass,} & 0 &= x+z+m, \\ &\text{From time,} & 1 &= -2y-z-2m. \end{aligned}$$

Now if we suppose $y = x$, as may very well be done, seeing that the flow is of a purely viscous nature, we are left with but one unknown, on account of peculiarities in the coefficients.

$$z = 1, \qquad m = -(x+1), \qquad n = 2x.$$

Thus

$$V^{-1} = \Sigma A\,(\mu \delta^x g^x S^{-(x+1)} D^{2x}).$$

Since the velocity increases with difference in density of the liquids, we give x the successive values -1, -2, -3, &c., and obtain the velocity expressed in a series.

As there are two coefficients of viscosity to be taken into account, the series should properly be of the form

$$V^{-1} = (a\mu + b\mu')\left\{\frac{A_1}{\delta g D^2} + \frac{A_2 S}{\delta^2 g^2 D} + \frac{A_3 S^2}{\delta^3 g^3 D^6} + \&c.\right\}.$$

* The form of the series represents the reciprocal, instead of the velocity itself, because it so happened the constants were originally so calculated, and a change would involve the arithmetical labour over again.

† The dimensions of δ are $\delta = M/L^3$, of $g = L/T^2$, of $\mu = M/LT$, and of $S = M/T^2$.

But in most of the experiments made the viscosity of the liquid of which the column was composed was so much greater than that of the bubble that the following form proved sufficiently accurate :—

$$V^{-1} = \frac{A_1\mu}{\delta g D^2} + \frac{A_2\mu S}{\delta^2 g^2 D^4} + \frac{A_3\mu S^2}{\delta^3 g^3 D^6} + \&c.$$

The value of these constant coefficients could be experimentally found by a series of determinations of velocity through different sized tubes, the same two substances being used throughout.

Taking only three terms of the series, I have done this for the case of air bubbles ascending through columns of glycerine of different diameters, and I find that the constants thus determined are practically *the same* as those required by my experiments with other substances. Thus three terms would appear to be ample.

In the following table is exhibited the time taken by a bubble of air to ascend 1 cm. (the reciprocal of the velocity) through a column of glycerine, the diameter of which is given in the top row.

Table I.—Air—Glycerine.

Diameter of tube..	0·609	0·665	0·775	0·895	1·03	1·28	1·46	1·68
Time observed....	40·5	15·2	7·43	3·09	1·73	0·784	0·522	0·325
Time calculated...	32·3	18·9	7·77	2·67	1·72	0·723	0·493	0·336

The third row was calculated by using the values of the constants given below, which were themselves deduced from the second row by the method of least squares.

$$A_1 = 1{\cdot}308 \,.\, g/\mu;$$
$$A_2 = 0{\cdot}02322 \,.\, g^2/\mu;$$
$$A_3 = 0{\cdot}0009108 \,.\, g^3/\mu.$$

The difference in density was 1·25, and the surface tension 63 dynes per centimetre. Temperature was that of the air, and ranged between 10° and 14°. For some sized tubes the agreement between the observed and the number calculated is not good, but this is probably due to variations in temperature. The viscosity of glycerine varies rapidly with temperature. The importance of constant temperature was not appreciated until most of the experiments made had been completed.

Having once determined the constant coefficients, it becomes possible to calculate the velocity of a bubble of any substance through a tube of glycerine of given diameter. The only things now requisite

for doing this are the difference in density of the bubble and its surface tension. An examination of the following tables will, I think, justify this assumption.

In Table II are exhibited the values of the "velocity reciprocal" calculated for chloroform in this way. That is to say, the ascertained values of the surface tension between chloroform and glycerine ($S = 12{\cdot}1$) and of the difference of their densities ($\delta = 0{\cdot}253$) were simply introduced into the expression we above obtained for the velocity. In the third row for comparison is given the time taken per centimetre found by actual experiment.

Table II.—Chloroform—Glycerine.

Diameter of tube.. ..	0·665	0·775	0·895	1·03	1·28	1·68
Time calculated.....	82·6	33·5	15·0	7·86	3·32	1·61
Time observed......	81·2	33·7	14·4	8·17	3·40	1·86

$\delta = 0{\cdot}253.$ $S = 12{\cdot}1.$

In the following two tables the same is given for creasote and mercury.

Table III.—Creasote—Glycerine.

Diameter of tube....	0·31	0·41	0·50	0·609	0·665	0·775	0·895	1·03	1·28
Time calculated.....	480	98·6	37·4	18·9	14·2	9·80	7·26	5·55	3·67
Time observed......	480	86·8	36·9	20·4	17·1	8·76	7·03	5·44	3·91

$\delta = -0{\cdot}199.$ $S = 2{\cdot}05.$

Table IV.—Mercury—Glycerine.

Diameter of tube........	0·41	0·50	0·665	1·03
Time calculated	12·6	3·15	0·495	0·097
Time observed..........	10·7	4·37	0·537	0·126

$\delta = 12{\cdot}34.$ $S = 370.$

For facility of comparison, the following table is selected from the foregoing, with the addition of one other, giving particulars in the case of two sized tubes of diameter 0·665 and 1·03 respectively.

TABLE V.

	δ.	S.	D = 0·665.		D = 1·03.	
			Time observed.	Calculated.	Time observed.	Calculated.
Mercury	12·34	370	0·537	0·495	0·126	0·097
Air................	−1·25	63	15·2	18·9	1·73	1·72
Oil of lemons	−0·377	6·8	12·0	12·7	..	..
Chloroform	0·253	12·1	81·2	82·6	8·17	7·86
Creasote	−0·199	2·05	17·1	14·2	5·44	5·55

The agreement with theory is remarkable, considering the great range introduced into the experiments. The density difference and the surface tension vary between themselves nearly 100 times.

In the accompanying diagram are plotted the various forms the equation takes on putting in the values of the physical constants proper to each body. The abscissæ represent the diameter of the tube in millimetres, and the ordinates the time calculated to be taken by the bubble in travelling 1 cm. The times found by experiment given in the above tables are marked so far as the limits of the paper would allow for the purpose of comparison.

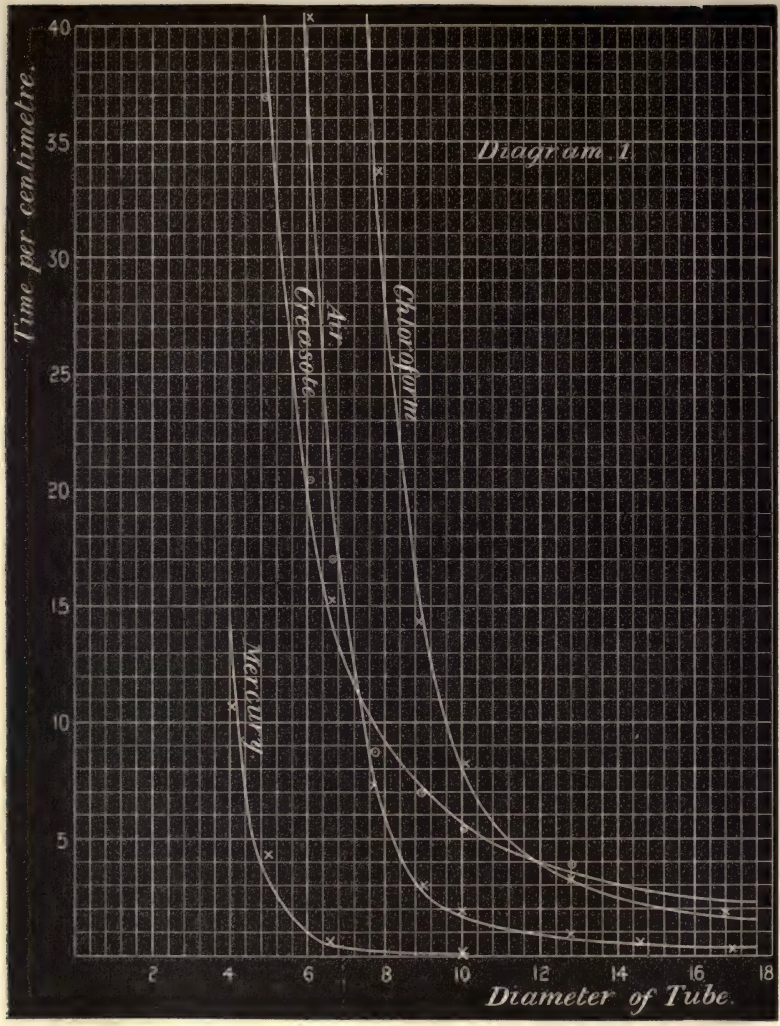
The case of creasote is instructive. With wide tubes the bubble moves comparatively slowly. For when the diameter is large, the first term in the series is most important, so that the density is then the controlling factor. But when D is small, the third term is most important, so that if the surface tension is very small, as is so in the case of creasote, the bubble will move comparatively quickly.

In such case the bubble assumes a long, almost needle-like appearance, giving ample room round it for the flow past of the liquid of the column. In the case of chloroform, for instance, this long form is not assumed because of the higher value of the surface tension.

The following table exhibits, in the case of several substances, the length of the bubble in motion relatively to its length at rest, when, of course, it occupies the whole width of the tube. The diameter of tube = 0·665.

Table VI.

Mercury	1·8
Air	1·2
Oil of lemons......	1·8
Chloroform	1·4
Creasote..........	2·0



Besides the foregoing, experiments were also made in which the viscosity of the liquid through which the bubble passed was changed in order to experimentally verify that the *velocity varied inversely as the viscosity* in agreement with the theory.

For this purpose the viscosity of glycerine was gradually reduced by the addition of water, and the "time of descent" of a chloroform bubble through a tube of the solution observed. This could be then compared with that calculated from the theory by introducing the value of the viscosity ascertained in each case.

In the following table are shown the observed and calculated "times" in the case of two sized tubes. The first column gives the approximate percentage composition of each solution, beginning with glycerine and ending with pure water. The fourth column consists of the ascertained viscosity in terms of that of water. Columns 5 and 6 are the calculated and observed "times of descent" respectively for a tube of diameter 0·655. Columns 7 and 8 are the like for a tube of diameter 0·895. Temperature between 10—12°.

Table VII.—Chloroform—Solutions of Glycerine and Water.

Per cent.	δ.	S.	μ.	D = 0·665.		D = 0·895.	
				Calc.	Observ.	Calc.	Observ.
100	0·253	12	833·0	82·6	81·2	15·0	14·4
90	0·277	14	199·0	20·5	19·1	3·73	3·35
80	0·294	*16* *	64·0	7·41	6·97	1·28	1·17
70	0·320	*18*	30·0	3·32	3·31	0·58	0·52
60	0·343	20	13·0	1·45	1·53	0·25	0·4
40	0·394	*23*	5·7	0·55	0·87		
0	0·404	28	1·0	0·08	0·62		

It will be seen from this table that within certain limits we are justified in assuming the velocity to be inversely as the viscosity, but there is a complete failure in the applicability of the law in cases where the viscosity is small. The retardation observed in such cases is doubtless due to the formation of eddying motions in the fluid, and, indeed, if any small motes which happen to be present be watched, considerable commotions are always to be seen as the bubble passes whenever there is this retardation.

The retardation is even more marked in the case of air bubbles, for as will be seen from the following table, not only are the observed values greater than the calculated values, but they are actually greater than values observed with *greater* viscosities, *i.e.*, than those given higher up the list. Diameter of tube = 0·665 c.c.

This retardation is better shown in Diagram 2, which is plotted from a number of careful observations. The maximum velocity with this sized tube corresponds to about 33 per cent. of glycerine, the velocity of which is about three times that of water at the temperature of the experiments. The abscissæ represent the percentage composition of the solutions, beginning on the left with glycerine. The ordinates are the corresponding velocities.

* The values of surface tension printed in italics were interpolated.

TABLE VIII.—Air—Solutions of Glycerine and Water.

Per cent.	δ.	S.	μ.	Time calculated.	Time observed.
100	1·251	63	833	18·9	15·2
90	1·227	64	197	4·94	4·48
80	1·210	65	64	1·70	1·67
70	1·184	66	30	0·88	0·88
60	1·161	67	13	0·41	0·58
40	1·110	69	5·7	0·22	0·48
35	1·101	70	3·6	0·15	0·45
0	1	74	1	0·06	0·53

It is of interest, as pointed out by Lord Kelvin,* to consider the increase in velocity accompanying increase in viscosity here exhibited in the light of Professor Osborne Reynolds' 'Critical Velocity.' As will be remembered, the 'Critical Velocity,' or the velocity of flow of a fluid just unaccompanied by eddying motions, rises in value with rise in the value of the viscosity. On the diagram, an hypothetical line to indicate the "critical velocity" for the bubble has been drawn. (That is to say, the velocity of the bubble which is accompanied by critical velocity of flow of the fluid past it.) All calculated values of the velocity below this line will stand. Above this line the observed velocity will lie between it and the line given by the calculation.

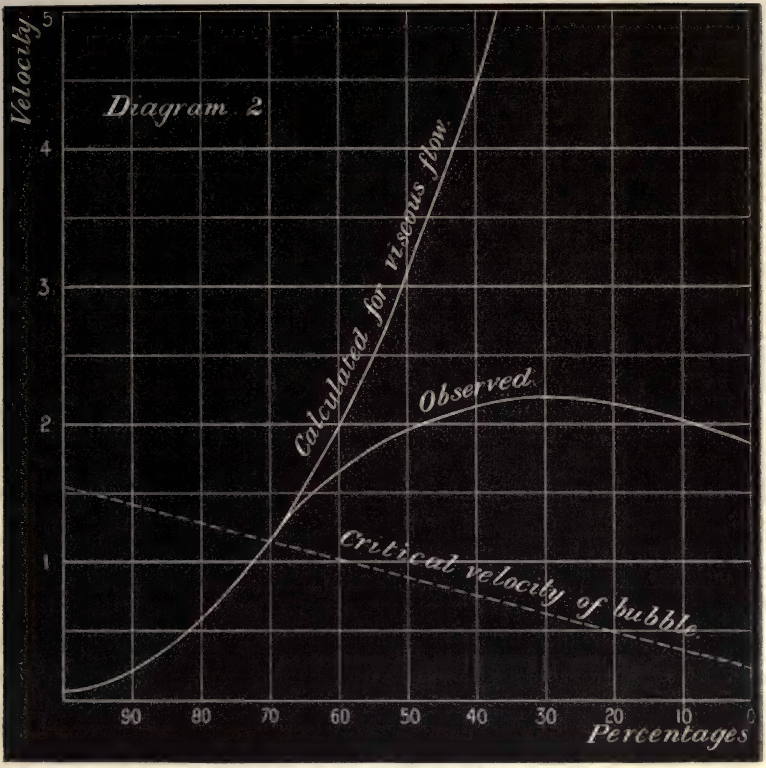
The form of the curve was found to be the same with solutions in water of sugar, of calcium chloride, or of sodium hydrate.

The actual turning down of the curve of velocities at small values of the viscosity, as occurs in these cases, would apparently be a matter of the rate of departure from each other of the critical velocity curve and that calculated from the theory of viscous flow, which latter, it must be remembered, depends on the surface tension and density as well as on the viscosity. These quantities probably have an important bearing in this particular question. By employing, say, gum tragacanth, one can increase the viscosity of water without sensible change in density.† Thus, in this respect the driving power is not simultaneously increased as is the case when increase in viscosity is produced by addition of glycerine.

With gum tragacanth the velocity never actually increases on increase of viscosity as in the case of glycerine. But through a considerable range in viscosity on the right-hand side of the diagram

* In the course of a discussion on a note read before the British Association in Edinburgh, 1892.

† The surface tension of these solutions changes in about the same relative ratio to the viscosity as solutions of glycerine in water do.



the velocity curve runs almost horizontal, showing clearly that the phenomenon of turbulency is present.

Similar curves to that for gum tragacanth were found on employing starch or gelatine to increase the viscosity of water.

The case of soap is remarkable. A very slight addition of soap to water produces quite a sensible increase in the velocity of the bubble. The first row in Table IX gives the percentage present of a certain soap solution ($\delta = 1{\cdot}026$) in mixture with water. Beneath is the velocity of the air bubble in each case. Diameter of tube was 0·661.

Table IX.—Velocity through Soap Solutions.

Per cent...	0	0·078	0·31	1·25	2·5	5·0	10·0	20·0	40·0	60·0	70·0	80·0
Velocity ..	1·86	1·90	2·39	3·68	3·76	4·13	4·18	4·25	4·46	3·52	1·26	0·74

It so happens that at first on addition of soap a diminution of viscosity does occur; nevertheless the chief cause in the increase in

velocity is probably due to the great diminution of the surface tension, which for the 5 per cent. solution was less than half that of water.

In connexion with this, as it probably depends on the formation of soap, must be mentioned a rather pretty experiment, which is easily made. A bubble of sweet oil if allowed to ascend a tube (say, of diameter = 1) of ordinary pure water passes up in the ordinary way. But, if the tube contains a weak solution of caustic soda, as the bubble ascends, the motion of the solution over the surface of the oil raises a series of circular waves round the cylindrical bubble, or, rather, a series of surface tension ripples. The waves almost invariably join at once to form one continuous spiral wave round the bubble, which then lends a surprisingly life-like appearance to the bubble as it wriggles its way upwards through the tube.*

The system of circular waves is evidently unstable, since the formation of the spiral means the opening of a continuous channel for the flow past of the solution.

The sign of the spiral (right- or left-handed) depends on initial circumstances, and can, when the tube is held in the hand, be conditioned at will by a judicious turn of the wrist.

The solution *must* be very weak, best about 1 part of strong caustic soda in 50,000 parts water; much stronger than this has too great a tendency to emulsify the oil, doubtless itself a phenomenon in part arising from diminution in surface tension on the more exposed parts of the oil.

When the liquid of which the column is composed is very viscous, the system of waves is prevented from forming, as is the case with a creasote bubble passing through glycerine, despite the fact of the surface tension being so very small.

At first sight it might appear that the ascent of bubbles through tubes of different liquids would prove a convenient method for comparing their viscosities. As has been seen, it is necessary, among other things, to know the surface tension in each case. This renders the comparison of viscosity in this way really a more troublesome process than by ordinary methods, since the determination of surface tension, especially if the liquids be viscous, is often accompanied by considerable uncertainty, owing to a persistent tendency to stick to glass frequently exhibited. This is particularly so in finding the surface tension between two liquids.

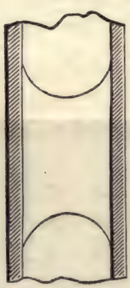
The phenomenon, however, would appear to be suited for the comparison of the surface tension between liquids. If the same liquid constitute the column in each case, it need not be necessary to know its viscosity, only the density being requisite.

* On inclining the tube, especially with smaller sized tubes, the bubble is seen to have quite a caterpillar mode of progression.

In this way the surface tension between two liquids, even though they dissolve each other in all proportions, may be measured. Thus, a bubble of water can be got to ascend a tube of glycerine, preserving for a considerable distance its perfectly distinct shape. In this fashion I have found 6·5 dynes per cent. for the initial value of the surface tension between water and glycerine. The velocity of the bubble rapidly increases as it proceeds, owing to the surface tension diminishing according as glycerine dissolves in the water. The continuous replacement of one of the liquids in the method gives it an advantage for such a purpose as this over any statical method. The quantity here discussed would in all likelihood prove to be related to molecular rates of diffusion, and perhaps should for this reason merit consideration.

An important point, which up to this has not been touched upon, is the necessity for the liquid of which the bubble is composed not to adhere so tenaciously to the walls of the tube as to cause the bubble to retain the shape shown in fig. 2 in section. This cannot

FIG. 2.



happen when the resolved component parallel to the axis of the tube of the tension of the surface between the liquids *plus* the tension at the surface of separation of the bubble and tube exceeds that between the second liquid and tube. Even though this be not the case, the bubble will often assume the shape with convex ends suitable for travelling through the tube.

With every substance, as the diameter of the column is reduced, a stage is reached when there is a tendency to stick even though the end be convex.

This limit is found to depend largely on the surface tension; with high values of surface tension it is soon reached. For instance, a bubble of mercury sticks hopelessly in a tube of glycerine of diameter 0·31, while creasote travels freely, although the pressure driving it is 62 times less. Now the surface tension in the case of mercury is 180 times that of the creasote used in these experiments.

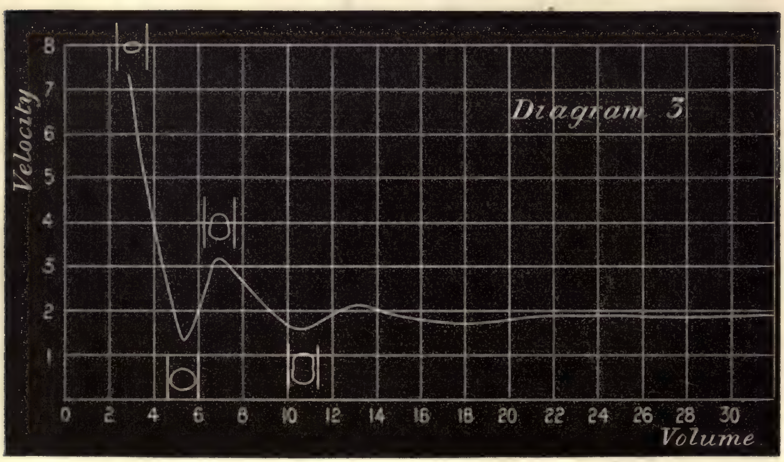
No quantitative determinations have been made on the remaining

experimentally unverified factor in our expression for the velocity of the bubble. That is to say to verify the terms under which the acceleration (g) appears in the expression. This, were it desirable, might of course be investigated by an arrangement utilising centrifugal force, but without elaborate arrangements it is not difficult to observe the great influence increase in pressure produced by swinging a tube round and round by hand has. To take an example, a bubble of creasote in a tube of glycerine of diameter = 0·31 takes over 13 hours to travel 1 m. By swinging it round and round in the hand, one can get it through in less than as many minutes.

In connection with this the comparative ease with which a stray liquid bubble in the tube of a thermometer can be brought home by swinging in the hand will suggest itself.

Part II.

When the length of the bubble is less than a certain thing, the velocity is found to be very different for different sized bubbles. The result of some experiments made with different sized bubbles of air passed up through a tube of water of diameter 0·665 is exhibited in Diagram III.



The ordinates represent the velocity and the abscissæ the volume of the bubble measured in terms of the length of tube it occupies when at rest. The length of the bubble in motion is always of course greater than this.

It will be seen that the velocity corresponding to a volume of about 7 is nearly double that at about 5·5. The phenomenon is, doubtless,

due to the form taken by the bubble. The figures on the diagram roughly represent the appearance of the bubble corresponding to the different points where they are placed. The small bubble on the left passes up rapidly. As larger volumes of air are taken to form the bubble the velocity falls to a minimum, at which point the bubble is almost spherical in shape. With further additions of air, the spherical form gives way to one pointed at top. Again more air swells out the top and gives the bubble a somewhat dumb-bell appearance. For this and the spherical form the velocity is a minimum. In these instances the ratio of the resistance of the annular channel to the flow-past of the liquid compared to the driving pressure is a maximum. The intermediate shape being pointed at top gains an increased pressure head without a corresponding increase in resistance. The various forms assumed by the bubbles remind one of the well-known initial shapes taken on the formation of liquid drops.

Similar curves were obtained with other sized tubes, the phenomenon being rather more marked in the case of the smaller sizes. Other liquids were found to behave in a like way when used either as the substance of the column or for constituting the bubble itself.

The curve in the diagram exhibits the general behaviour of the bubbles, but occasionally an anomalous determination of velocity will be obtained; this is accompanied by the bubble having also an anomalous form. That is to say, a volume of air a little in excess of that corresponding to the spherical stage, instead of assuming a pointed form at top, will retain a spherical form, and as a consequence will travel very slowly.

V. "On the Metallurgy of Lead." By J. B. HANNAY, F.R.S.E., F.I.C. Communicated by Sir G. G. STOKES, F.R.S. Received April 15, 1893.

[Publication deferred.]

Presents, *June* 1, 1893.

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June 8, 1893.

The LORD KELVIN, D.C.L., LL.D., President, in the Chair.

Professor William Burnside, Professor Wyndham R. Dunstan, Mr. William Ellis, Professor J. Cossar Ewart, Dr. Ernest William Hobson, Sir Henry Hoyle Howorth, Mr. Edwin Tulley Newton, Dr. Charles Scott Sherrington, Mr. John Isaac Thornycroft, Dr. Alfred Russel Wallace, and Professor Sydney Young were admitted into the Society.

A List of the Presents received was laid on the table, and thanks ordered for them.

Pursuant to the notice given at the last meeting of the Society, the President proposed and the Senior Secretary seconded H.R.H. the Duke of York for election and immediate ballot. The ballot having been taken, His Royal Highness was declared duly elected a Fellow of the Society.

The following Papers were read:—

I. "Preliminary Report of the Joint Solar Eclipse Committee of the Royal Society, the Royal Astronomical Society, and the Solar Physics Committee on the Observations of the Solar Eclipse of April 16, 1893." By A. A. COMMON, F.R.S. Received June 7, 1893.

The Joint Committee have requested me to make the following brief report on the observations of the Eclipse. This will be followed shortly by a more complete report.

The Joint Committee was formed early in 1892, a grant of money was obtained from the Government Grant Fund of the Royal Society, and preparations were at once begun. After due consideration, it was decided to send out two observing expeditions, one to Fundium, on the Salum River, in Senegambia, and one to Pará-Curu, in the Province of Ceará, in the northern part of Brazil. With the exception of the work undertaken by Professor Thorpe, the whole of the observations were photographic. Three classes of work were undertaken at each station.

1st. Photographs of the corona, in continuation of a long and very complete series already taken with the "Abney" lens, and similar

photographs, on three times the scale, by means of a negative enlarging lens by Dallmeyer.

2nd. Photographs of the surroundings of the Sun by means of a prism in front of the object glass (prismatic camera).

3rd. Photographs of the spectrum of the corona by slit spectroscopes.

The West African Expedition was placed in charge of Professor Thorpe, F.R.S. Professor Thorpe, assisted by Mr. Gray and Mr. Forbes, undertook the determination of the photometric intensity of the coronal light on the method he used at the Solar Eclipse of 1886, at Granada. A complete and satisfactory number of observations were made.

Mr. A. Fowler undertook the prismatic camera observations, using a 6-in. telescope, lent by Mr. Lockyer, with a large prism in front of the object glass. Mr. Fowler took six plates before and after totality, and fifteen during totality. The photographs are considered by Mr. Lockyer, at whose wish this investigation was made, to be of very great value.

Sergeant Kearney, R.E., had charge of the coronagraph. With the Abney and Dallmeyer lenses and a double camera, eleven pictures of the corona were secured, and these are of a most satisfactory character.

Captain E. S. Hills, R.E., undertook the slit spectroscopes, and obtained two excellent photographs.

Mr. A. Taylor and Mr. Shackleton formed the expedition to Brazil. The coronagraph was placed in the charge of Mr. Taylor, as well as the slit spectroscopes, to be used if the necessary local help could be obtained. Twelve photographs of the solar corona were obtained, of a similar character to those obtained in Africa, and directly comparable with them as regards exposure, density, and detail of the coronal structure. Most of these coronal plates have Captain Abney's density squares impressed on them for determining the density of the photographic image. Two photographs with the slit spectroscopes were obtained.

Mr. Shackleton, with an arrangement somewhat similar to that employed by Mr. Fowler, took a large number of photographs; these are only less valuable than the African photographs in that the instrument employed was on a smaller scale.

The air at Fundium was hazy. At Pará-Curu the observations were made under peculiarly fortunate circumstances, as the Sun was clear of clouds only for a short time about the time of the Eclipse.

Generally speaking, the results obtained are of a most satisfactory character. The photographs taken at each station provide a large amount of material to work upon, particularly those by the prismatic camera. From the distance apart of the two stations and the dupli-

cation of the work, a comparison may throw some light on the question of change of form and nature of the surroundings of the Sun during the interval between the observations. In this respect we may have the photographs taken in Chili to further extend this time interval.

The various members of the expeditions have enjoyed good health, and no one seems to have suffered injury from the excessive heat.

The Committee are under great obligations for much assistance given to the expeditions. The work of observation in Africa was made on French territory. The French Government did everything possible in granting a choice of sites, and M. Victor Allys, the French Administrator at Fundium, gave most valuable help.

The Admiralty have given us a gunboat to take the party up the Salum River and attend on them during the time this work lasted, and a cruiser brought the party from Bathurst to Grand Canary. The value of the help afforded by the Admiralty can be appreciated when it is known that without it this expedition could not have been sent.

From many other quarters most valuable aid has been received, and will be more fully acknowledged in the General Report.

II. "On the Bright Bands in the present Spectrum of Nova Aurigæ." By William Huggins, D.C.L., LL.D., F.R.S., and Mrs. Huggins. Received May 29, 1893.

Some few prefatory words are called for in explanation of the partial incompleteness of the present communication.

A considerable brightening, from below the 14th magnitude to above the 10th magnitude, was found to have taken place in the Nova when it was re-observed in the early part of August, 1892, and to be accompanied by a modification of its spectrum, apparently analogous to a similar change in the spectrum of Nova Cygni in 1877, since the observations we made of the star on March 24, 1892, when it had fallen to nearly the 11th magnitude.*

In consequence, however, of the removal of the eye-end of the telescope to the workshops of Messrs. Troughton and Simms for the attachment to it of the mounting for a fine Rowland grating by Mr. Brashear, we were without the means of observing the star and its spectrum during the whole of the autumn and the early winter. It was not until the beginning of the year that the new spectroscope was mounted in our observatory, and then, from some instrumental causes of delay and from a prevalence of bad weather, we were

* 'Roy. Soc. Proc.,' vol. 51, p. 492.

not able to observe the spectrum of the Nova until the night of February 1.

Before this time the altered appearance of the spectrum of the Nova had been observed at several observatories, and its spectrum had been described as consisting mainly, in the visible region, of a bright line in the orange, of the two nebular lines, and of the hydrogen line at F.

As soon as we directed the spectroscope to the star, we saw at once, even with one prism, that the two principal bright bands which had been described as the "nebular lines" were, in strong contrast with these, not single lines but broad bright spaces, diffused at the ends and irregularly bright, which we suspected to be groups of bright lines.

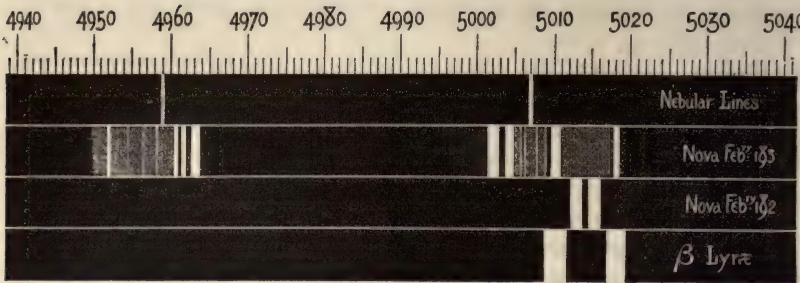
On February 8 we observed these bright spaces with the 4-inch grating of 14,438 lines to the inch, using the spectrum of the second order. The collimator and the telescope have each an aperture of 2 inches, and the spectrum was viewed under a magnifying power of 23 diameters. Our suspicion was then confirmed, the bands being clearly resolved into groups of bright lines upon a feebly luminous background.

On February 26, micrometric measures were begun of the positions of the constituent lines of the groups, when unfortunately we discovered that in consequence of flexure in one part of the instrument, a shifting of the micrometer webs relatively to the lines of the spectrum was liable to take place, and so make the measures uncertain to about as much as 2 tenth-metres when the spectrum of the second order was in use.

The cause of the want of rigidity of the instrument in this respect made it necessary that the spectroscope should go back to Messrs. Troughton and Simms' workshops; and then, from unavoidable delays and the coming in of the Easter holidays, it was not until the second week in April that the spectroscope was again in position for use; but by this time the Nova was too far past the meridian for satisfactory observations to be made upon its spectrum.

Our opportunities of working upon the spectrum of the star were thus absolutely restricted to the few fine nights between February 1 and February 26; and, further, our observations of the positions of the lines are, for the reason we have mentioned, affected with a possibility of error which may be as great as 2 tenth-metres, though it is probable that the positions given in the diagram are not actually in error to as much as half that amount.

For the same reason the resolution of the minor features of the groups has not been worked out with the completeness which was well within our instrumental means, if the number of fine nights had not been so limited, for on some of the nights on which observations



were attempted the sky was not clear enough from thin haze for the resolution of the more difficult features of the spectrum of a star of between the 9th and 10th magnitude.

Still, notwithstanding the comparatively incomplete state of our observations, which we greatly regret, we do not hesitate to consider them of sufficient importance, bearing as they do upon so remarkable a phenomenon as would be the change of a star into a nebula, to justify us in communicating them to the Royal Society.

The spectroscope is provided with a 4-inch Rowland grating by Brashear, and a prism of dense flint of 27°, silvered on one face, which can take the place of the grating in the grating box.

As we have already stated, the observation of the Nova with this prism showed the bright "lines" broad and irregularly bright, and raised the suspicion in our minds that they were probably groups. They were observed more or less successfully with the grating, usually with an eye-piece magnifying 23 diameters, on February 8, 10, 11, 16, 17, and 26.

1. *Brighter Group near the Position of the Principal Nebular Line.*

The separate results of our more favourable observations of this group on the different nights are put together in the accompanying diagram. In addition, however, to the details drawn in the diagram, at several very favourable moments of seeing, we had distinct and undoubted glimpses of finer lines in the spaces between the brighter ones, of which some only are given in the diagram. For this reason the diagram must be regarded as an incomplete representation of the group, though showing accurately its main features and general character.

The group, as shown in the diagram, extends through a little more than 15 tenth-metres, and consists of lines more or less bright upon a feebly luminous background, which can be traced to some distance beyond the lines at both ends of the group. The more prominent features are: two lines, the brightest in the group and about equally bright—but the more refrangible one rather the

brighter—which form the termination of the group towards the blue; a line nearly as bright about the middle of the group; and a third prominent line at the end of the group towards the red.

We have little doubt, though we hesitate to state it positively, that the space between the two brightest lines, that on the blue side of the bright line in the middle of the group, and the spaces on the blue sides of some others of the lines, were darker than the faint luminous background, in which case we should have to do possibly with lines of absorption of the same substances shifted towards the blue. A few only of the finer bright lines which were occasionally glimpsed between the more brilliant lines have been put into the diagram.

The pair of bright lines at the termination of the group towards the blue makes this the brighter end of the group, which does not, however, as a whole possess any of the usual features of a fluting.

On February 10, the micrometer webs were placed so as just to include the bright lines of the group, but not the faint background which at the clearest moments could be traced for some distance, especially at the blue end of the group. The instrument remained untouched, and the position given in the diagram is that found from the places of the micrometer webs upon the solar spectrum, on Rowland's scale, as observed on the following morning.

On the 26th measures of this group were made relatively to the position of the principal line in the nebula of Orion; these gave also almost exactly the same position in the spectrum for the group, but, as we have already stated, all these measures are unfortunately liable to a small error from the possible flexure, at that time, of a part of the instrument.

The mean of Mr. Campbell's measures at the Lick Observatory, during the period of our observations, from February 10 to February 27, gives λ 5006 for the middle of the band. He remarks: "In any discussion of these observations it is necessary to take into account the difficulty of accurately locating the centre of a line so broad and diffuse as this one is."*

In another place Mr. Campbell says: "The line is at least 8 tenth-metres broad and the edges very diffuse."†

These observations would be brought into accordance with our own, so far as relates to the length and the position of the band, if we suppose Mr. Campbell to have observed only the more refrangible and much brighter half of the whole group.‡

* 'Astronomy and Astro-Physics,' May, 1893, pp. 418, 419.

† 'Publ. Ast. Soc. Pacific,' vol. 4, p. 246.

‡ Professor Campbell also says: "On August 30 the line was suspected to be double, and the grating measures of that night refer to a point midway between the two condensations. On September 7 the measures refer to a point of maximum brightness slightly less refrangible than the centre of the line."—'Astronomy and Astro-Physics,' Oct., 1892, p. 718.

The probable analogy between the Nova and the remarkable variable star β Lyræ, in the spectrum of which also we have to do apparently with bright and dark lines of the same substances, though not in all cases identical with those of the Nova, in motion relatively to each other, which we ventured to point out in our former communication on the Nova,* has been recently greatly strengthened by the photographic observations of β Lyræ at different stages of its periodic variations by Dr. Bélopolsky at the Observatory of Pulkova.

In some of his photographs, especially in one taken shortly after the star's second maximum, bright lines come out near the positions of the bright groups of the Nova which are now under discussion. As the scale of Dr. Bélopolsky's photographs is much smaller than that of our diagram, we felt some hesitation in attempting any identification of his lines with those of the Nova. At our request, Dr. Bélopolsky has been so kind as to put into our diagram the two brightest of the lines of β Lyræ, as they appeared shortly after a second maximum, which fall within the brightest group of the Nova, and which, indeed, may be identical with two of the lines in the Nova. It may, however, be thought that the lines of β Lyræ suggest that they are independent bright lines rather than members of a group such as that of the Nova.

Whatever may ultimately be found to be the truth, there can be no question as to the probable high significance of the remarkable analogy which exists between the changes which take place in β Lyræ and those which have been observed in Nova Aurigæ.

The two other spectra in the diagram represent respectively the position and character of the two nebular lines, and the position of the bright double or multiple band which was so brilliant in this region of the Nova in the beginning of last year.

2. *Bright Group near the Position of the Second Nebular Line.*

Not anticipating that our opportunities of observing were to be so soon cut off, we gave our attention chiefly to the brighter group, intending, after we had completed our observations and measures of it, to attack seriously the second group.

However, on nearly all the nights we observed we gave some attention to this group, which, from being fainter, is more difficult to resolve, though on the clearer nights it was fairly well seen with the grating.

Generally, the group may be described as of the same order as the brighter one, consisting of bright lines and possibly of some absorption lines upon a feebly illuminated background.

* 'Roy. Soc. Proc.,' vol. 51, p. 494.

We have endeavoured to represent in the diagram as truthfully as we can the best views we obtained of this group; but during one or two exceptional moments of good seeing we thought that we glimpsed finer bright lines in the spaces between. Indeed, the group may consist of a close grouping of bright lines.

For the same reasons, fewer measures were attempted of this group, and its position was less accurately determined, but neither the constitution of the group as represented in the diagram nor its position can, we think, be much in error.

We were also unable to work upon the bright line in the orange, and to do more than satisfy ourselves, by a direct comparison, that the line about F was really the hydrogen line in that region.

General Conclusions.

It need scarcely be said that no contrast could well be more striking than that which these extended groups of lines form with the two narrow and defined lines in the spectrum of the Great Nebula in Orion.

It is difficult to suppose that we have to do with the same substance or substances, whatever they may be, which produce the nebular lines, even if we imagine very different conditions of temperature, or even allotropic conditions.

In the laboratory, allotropic changes are not usually accompanied by new groups, or lines at the positions of the characteristic lines of the substances in their original state.

We wish to speak at present with great reserve, as our knowledge of the Nova is very incomplete, but we do not regard the circumstance that the two groups of lines above described fall near the positions of the two principal nebular lines as sufficient to show any connexion between the present physical state of the Nova and that of a nebula of the class which gives these lines.

Influenced by the analogy between some of the changes in the spectrum of the Nova and those which are associated in the spectrum of β Lyræ with the variation of its light, and also by other reasons which we pointed out in our former communication, we are still strongly inclined to take the same view which we there ventured to suggest, namely, that in the outburst of the Nova we have not to do mainly with cold matter raised suddenly to a high temperature by a collision of any form but rather, for the most part, as was suggested by Dr. Miller and myself in 1866 in the case of the first temporary star examined with the spectroscope, with an outburst of existing hot matter from the interior of the star or stars; indeed, with phenomena broadly similar to, but on an immensely grander scale than those with which we are familiar in the periodic greater and lesser disturbances of the Sun's surface.

Such grand eruptions may well be expected to take place as stars cool, and if in two or more dull and comparatively cool stars such a state of things were imminent, then the tidal action due to their near approach might be amply adequate to determine, as by a trigger action, such eruptions.

Under such conditions, fluctuations of brightness and subsequent partial renewals of the eruptive disturbances might well take place.

III. "The Process of Secretion in the Skin of the Common Eel." By E. Waymouth Reid, Professor of Physiology in University College, Dundee. Communicated by Professor M. Foster, Sec. R.S. Received April 18, 1893.

(Abstract.)

Leydig, more than forty years ago, demonstrated the possibility of a secretory process in the skins of Fish by the discovery in the epidermis of some twelve genera, of specialised cells to which the name of "schleimzellen" was given. Since then Kölliker, Max Schultze, F. E. Schulze, Foettinger, List, Leydig himself, and others have extended our knowledge of the anatomical secreting elements of the epidermis, and shown that in many instances it is extremely probable that several varieties of such structures exist. Of the several forms of glandular elements, the goblet cell is the most widely spread, and its epidermic origin and development has been most carefully investigated by F. E. Schulze and List. Considerable difference of opinion has, however, arisen regarding the function of another form of specialised epidermic cell, viz., the club cell ("kolben" of Max Schultze), which was originally described by Kölliker for *Myxine* and *Petromyzon*, though F. E. Schulze found that such cells also occurred in *Tinca*, *Leuciscus*, *Cobitis*, *Esox*, *Silurus*, and *Anguilla*, and Fritsch in *Malapterurus*. Kölliker himself, in *Myxine*, recognised the relationship of these cells to the thread cells of the mucous sacs, first clearly described by Johannes Müller. Max Schultze, however, deemed them to be of the nature of nervous end organs, possibly contractile, on account of certain appearances in polarised light recalling those of striated muscle fibre. H. Müller, F. E. Schulze, Foettinger, Leydig, and Fritsch, finding that these club cells are not constantly found in contact with the corium, as Max Schultze thought, have all inclined towards considering a secretory function probable for these structures, but have given no very definite information as to its details. Quite recently Pogojeff has again upheld Max Schultze's nerve end organ theory in the case of *Petromyzon*.

Convinced that much of the variation in description of the appearances of the secretory elements, and of the club cells in particular, had arisen from the fact that no special care seems to have been taken by any of the above observers to note the condition of the skin as regards secretory activity at the time of fixation for histological work, I have paid special attention to the condition of my fish, and have also resorted to artificial methods of excitation.

The Eel possesses both goblet cells and club cells in its epidermis, and is therefore suitable for the study of the process of secretion in both of these elements.

To obtain skins in the lowest phase of secretory activity, hybernating fish were obtained, rendered motionless by a successful transfixion of the medulla, and the skin immediately removed before the condition of "shock" had passed off, and therefore without any reflex movement and concomitant secretory action. Such skins are termed "normal." To obtain skins in the highest phase of secretory action, the headless or intact fish (usually caught in summer) was either exposed to the action of the vapour of chloroform, which acts at first as a powerful stimulant, subjected to faradisation, or allowed to writhe and slime in the manner common to Eels. Skins from such fish are designated "stimulated."

An examination of the *slime* of an Eel reveals the following histological elements:—

Fibres from 2 μ in breadth to the finest fibrils, inexcitable by electricity, indigestible by acid pepsin or alkaline trypsin, giving the xanthoproteic reaction, and staining brilliant yellow with picrocarmine. They are probably chemically of the nature of keratin. These fibres resemble in microscopic appearance those of the slime of *Myxine*, and from Eels placed in baths of pilocarpine solution are often obtained in convoluted masses.

Granules from 0·5 μ to 0·75 μ in diameter, soluble in 5 per cent. acetic acid, giving the xanthoproteic reaction, swelling, but not dissolving, in dilute alkali, resisting peptic and tryptic digestion, and staining red with picrocarmine.

Nuclei from 2 μ to 4·5 μ in diameter.

Epidermic cells, and occasionally extruded *goblet cells*.

In addition, mucin is present in the slime, seeing that the aqueous extract boiled for several hours with 2 per cent. sulphuric acid yields a substance capable of reducing Fehling's fluid, and is moreover precipitated by acetic acid. The acetic acid precipitate, however, being partly soluble in excess of acetic acid, and leaving an insoluble residue after digestion with pepsin and hydrochloric acid, it is probable that nucleo-albumin may also be present, though sufficient material for a phosphorus analysis was not collected.

Histology of the Normal Epidermis.—This has been studied by

maceration in Ranvier's "third part" alcohol and teasing, and by sections cut by the paraffin method.

The club cells arise from the cells of the palisade layer by amitotic division. Around the nucleus of the young cell a granular modification of the protoplasm occurs, which is the forerunner of the formation of a vesicle which always bears a distinct relation to the nucleus in its origin. The contents of the vesicle, at first homogeneous, become granular, and a lattice work of the surrounding protoplasm forms a distinct wall. In staining reaction the contents of this vesicle differ markedly from those of the goblet cells described below. They refuse to give the red-violet reaction with thionin, considered by Hoyer as characteristic of mucin, stain well with soluble blue, alone of all the dyes used, except that in sublimate specimens they take the methyl green of the Biondi stain in contradistinction to the body of the club cell which takes the acid fuchsin.

At a later stage, by a vacuolation of the material of the club cell round about the vesicle, it is set free, either as a cell with latticed wall resembling the "Leydig's cells," described by Leydig, Langerhans, Flemming, and Pfitzner in the epidermis of larval *Proteus* and *Salamander*, by List in larval *Triton*, and Carrière and Paulicki in *Siredon pisciformis*, or as a granular mass with some of the club cell body material still adherent. (Formation of the "escape cell" or "escape mass.")

The remainder of the club cell forms a spirally coiled mass ("fibre mass") staining brilliant yellow with picrocarmine.

Both "escape cell or mass" and "fibre mass" finally reach the surface of the epidermis and are extruded, the granules and nuclei of the former giving rise to the granules and nuclei of the slime, and the latter becoming further broken up to supply the fibres.

In the elimination of the elements derived from the club cells the surface epidermis is lifted, and the spiral formation of the fibre masses appears to aid in this act.

The goblet cells are of the "footed" variety, and arise direct from the palisade cells. They are pushed to the surface by the supply of ordinary epidermic cells originating below. The young cells, with closed theca in the lower layers, contain distinct granules in osmic vapour or Flemming's fluid material, giving the red-violet reaction with thionin and staining well with most basic dyes. There is no evidence of List's "filar and inter-filar mass," except in those cells which have nearly reached the surface, and whose contents are probably altered by imbibition. A process of regeneration of goblet cells near the surface appears to occur, for after the discharge of the first load of mucigen the protoplasmic foot remaining in the epidermis grows, and develops the red-violet thionin reaction never present in the fully developed goblet, except in the contents of the theca.

Fibroblasts.—Among the cells of the lower layers of the epidermis are found small cells 4 to 5 μ in diameter resembling lymphocytes, generally in little masses, and with nuclei often presenting mitotic figures. Such cells have been described by Langerhans in *Petromyzon Planeri*, List in *Cobitis*, and Fritsch in *Malapterurus*. List has described them as wandering cells passing in from the corium, and considers that they are finally extruded in degenerated form, as Stohr has demonstrated in the case of the tonsil. Fritsch saw no evidence of this extra-epidermic origin in *Malapterurus*, and came to the conclusion that these cells supply the surface epidermic scales. In the Eel these cells are undoubtedly in their origin foreign to the epidermis, and can be traced from the blood vessels of the corium through the basement membrane. In the epidermis itself all forms can be traced between the lymphocyte-like cell (fibroblast) and connective tissue cells with fine processes, which abound especially in the lower layers. Leydig has already demonstrated that connective tissue cells other than chromatophores may exist between the epidermic elements of Fish (*Cyprinus carassius*), and it is interesting to note that Langerhans, who first described these "kleine Rundzellen," was of opinion that they represented contracted chromatophore-like cells devoid of pigment. It is easy to demonstrate a complete network of connective tissue in the epidermis by sections parallel to the surface, especially in cases where the bodies of the club cells have become shrunken, and its function appears to be to hold together a tissue which, on account of the peculiar processes involved in secretion, would otherwise be of very labile nature.

The process of secretion, therefore, so far as it can be deduced from the appearances in the epidermis of slowly secreting winter Eels, appears to be as follows :—

Goblet cells, the direct descendants of palisade cells, are gradually forced to the surface by young epidermic cells derived from the same source. On nearing the surface these swell, probably by imbibition of water, and, a stoma forming in the theca, the contents are discharged, the remainder of the cell not necessarily being at once extruded, but capable of undergoing regeneration. The club cells from the same origin end in (i) a spirally wound fibre mass, which, after probably helping, by a kind of "elater" action, to remove the surface, is discharged and breaks up into the slime fibres; and in (ii) a mass of granular material inclosing the original nucleus, also discharged, and giving rise to the granules and nuclei of the slime.

Histology of Artificially Stimulated Epidermis.—The details of the secretory act deduced from the observation of the slowly acting skins of winter Eels are confirmed, and in some points extended, by the observation of stimulated skins.

Chloroform vapour applied to the headless or intact animal causes

such violent action that the whole epidermis is loosened. An almost "volcanic" eruption of "fibre masses" from the club cells occurs, and at the same time many new goblet cells appear in the lower layers. This result, so far as concerns the club cells, is of reflex origin, for chloroform vapour applied to the exsected skin does not produce the effect. There is, then, physiological evidence of a connexion between the club cells and the central nervous system, though I have been unable to obtain convincing proof of actual nerve fibrils by the use of gold. The surface of the epidermis may be completely thrown off by the rapid production and uncoiling of the "fibre masses" of the club cells. At the same time a rapid passage of fibroblasts into the epidermis takes place, probably with a view to affording support to the epidermic elements during the subsequent regenerative processes that must occur. This inroad of fibroblasts may be so great, and the secretory activity so violent, that, under such circumstances, whole masses may be extruded still in the elementary lymphocytic form.

The action of faradisation is less violent, but, not being followed by any narcotic action upon the elements, may be employed to obtain a picture of the result of prolonged stimulation. In such experiments the epidermis is found bereft of superficial cells, and its surface covered by a mass of extruded club cells and fibre masses, if the stimulation has been carried out in air. Dividing nuclei in both the palisade cells and ordinary epidermic cells are far more frequent than normal, and, as before, the number of fibroblasts is excessive.

By poisoning Eels with atropine a condition of the epidermis is found in which the club cells go through their metamorphosis while still in contact with the corium, the surface epidermis is intact, and the whole structure becomes thicker from the formation of epidermic cells without concomitant removal.

In conclusion, it should be noted that, by stimulating specimens of *Petromyzon fluviatilis* with chloroform vapour, evidence has also been gained of a similar production of fibres from the bodies of the club cells. The conclusions stated at the end of the communication of which the above is a short abstract are as follows:—

1. The secreting elements of the epidermis of the Common Eel consist of goblet cells and club cells, both direct descendants of the cells of the palisade layer. The former supply a mucin, the latter threads and a material appearing as fine granules in the slime.

2. The goblet cells contain mucin granules, and, after reaching the surface and discharging their load, are capable of undergoing regeneration by growth of the protoplasmic foot and re-formation of mucin.

3. The threads of the slime resemble those of *Myxine glutinosa*,

but are usually of finer texture. As in *Myxine*, they are developed from the club cells, but there are no special glandular involutions of the epidermis. The club cells of *Petromyzon fluviatilis* also supply slime threads.

4. The granular material of the slime is the contents of vesicular spaces developed in the club cells in the immediate neighbourhood of their nuclei, and is set free enclosed in a lattice work developed by vacuolation of the surrounding material, and finally extruded, carrying with it the original nucleus of the club cell.

5. The remainder of the club cell, after extrusion of its vesicle and nucleus, becomes a spirally coiled fibre, which finally breaks up into the fine fibrils of the slime.

6. Severe stimulation, especially by the vapour of chloroform applied to the intact animal, causes so sudden a development of the coiled fibres from the club cells that the surface of the epidermis is thrown off and the secretory products set free *en masse.* This process is of reflex nature, for similar excitation applied to excised skin is without effect.

7. A system of connective tissue cells, distinct from chromatophores exists in the epidermis developed from cells which are direct descendants of leucocytes, and which can be traced from the blood vessels of the corium through the basement membrane into the epidermis. The number of these wandering cells in the epidermis is greatly increased by stimulation, probably with a view to providing subsequent support to the secretory elements during regeneration.

IV. "The Experimental Proof that the Colours of certain Lepidopterous Larvæ are largely due to modified Plant Pigments, derived from Food." By E. B. POULTON, F.R.S. Received May 12, 1893.

(Abstract.)

The object of this investigation was to afford a conclusive test as to the theory, previously submitted by the author, that some of the colours of certain Lepidopterous larvæ are made up of modified chlorophyll derived from the food-plant.

Larvæ from one batch of eggs laid by a female *Tryphœna pronuba* were divided into three lots fed (in darkness) respectively throughout their whole life upon (1) green leaves, (2) yellow etiolated leaves, and (3) white mid-ribs of cabbage. The larvæ fed upon (1) and (2) became green or brown as in nature, thus proving that etiolin, no less than chlorophyll, can form the basis of the larval ground-colour. Those fed upon (3), in which neither chlorophyll nor etiolin

was accessible, were entirely unable to form the green or brown ground-colour. The production of dark superficial cuticular pigment was, however, unchecked. One of the larvæ fed in this way was perfectly healthy, and had become nearly mature when it was accidentally killed. Many others died early, but resembled that last described in the inability to form a ground-colour.

The experiment seems to leave no doubt as to the validity of the conclusions previously reached. Interesting questions as to the changes passed through by the derived pigments are suggested by this inquiry.

V. "The Influence of Exercise on the Interchange of the Respiratory Gases." By W. Marcet, M.D., F.R.S. Received May 18, 1893.

I had the honour of communicating two papers to the Royal Society on the interchange of pulmonary gases—one in June, 1891,* and another in June, 1892.†

The methods adopted and instruments employed have been fully described, and as the present inquiry is a continuation of the former investigation, carried on by similar methods and with the same instruments, there will be no necessity to refer to either of these on the present occasion.

It might, however, be stated that the expired air was collected in a bell-jar of a capacity of 40 litres, and over salt water; the CO_2 was determined by Pettenkofer's method, and the O by means of an eudiometer of a special construction. A short historical sketch of the work done on the subject under consideration has been given in the previous papers.

I have been very ably assisted in the present inquiry by Mr. Bernard F. Davis, B.Sc., who kindly submitted to experiment, and carried out for me, with every care, many determinations of carbonic acid and oxygen.

The object of the present communication is to show the influence of exercise on the interchange of pulmonary gases, but I must beg leave to preface the subject with a few remarks.

It has occurred to me that the words "interchange of respiratory gases" might not at first sight carry with them a perfectly clear meaning. The word "interchange" obviously refers to the movement of two gases exchanging places, and this applies to the passage of the oxygen of the air into the blood through the substance of the lungs,

* 'Proceedings Roy. Soc.,' vol. 50.

† 'Proceedings Roy. Soc.,' vol. 52.

and of the carbonic acid of the blood into the air of the pulmonary cavity also through the pulmonary tissue. The word "interchange" might be thought to mean that a certain volume of oxygen is exchanged for an equal volume of CO_2, but such is not the case, as the volume of oxygen taken up by the blood, and said to be consumed, is larger than the corresponding volume of CO_2 emitted, the difference being due to the O absorbed, which may be considered as employed in the phenomena of "tissue-change."

During ordinary respiration in a perfect state of repose, and under similar circumstances relating to temperature and food, the formation of CO_2 and absorption of O in the same person alters within certain limits. Together with these changes there is a marked tendency for the oxygen consumed (CO_2 produced and O absorbed) to assume a constant figure in the same person, or it may be said that there is a marked tendency to a decrease of CO_2 expired being accompanied by an increase of oxygen absorbed, and *vice versâ.* Four different persons were experimented upon, and this tendency is very clearly shown on three in the accompanying table, in which every figure is the mean of the two readings most alike.

In the fourth case, that of my present assistant, Mr. B. F. Davis, the tendency is of a different kind.

On a close consideration, the phenomenon observed on the three first persons experimented upon admits, I think, of the following explanation:—

If the carbonic acid expired should suddenly increase, it does so at the expense of the oxygen absorbed, and there is less of it left for the purposes of tissue-change; hence it is observed to diminish at the same time as the CO_2 is increased.

Table showing the tendency of the Oxygen consumed to remain constant under similar circumstances.

The Author.

Under influence of food.			Fasting.		
CO_2 expired.	O absorbed.	O consumed.	CO_2 expired.	O absorbed.	O consumed.
228·0 c.c.	26·0 c.c.	254 c.c.	207·2 c.c.	31·0 c.c.	238 c.c.
220·0 „	29·0 „	249 „	202·6 „	32·2 „	335 „
206·7 „	35·8 „	242 „	192·9 „	42·5 „	235 „

Mr. Russell.

Under food.			Fasting.		
CO_2 expired.	O absorbed.	O consumed.	CO_2 expired.	O absorbed.	O consumed.
305·1 c.c.	27·4 c.c.	332 c.c.	255·1 c.c.	25·5 c.c.	281 c.c.
292·7 „	42·3 „	335 „	241·3 „	38·4 „	280 „
282·0 „	49·6 „	332 „	226·2 „	36·2 „	262* „

Mr. D. Smith.

Under food.			Fasting.		
CO_2 expired.	O absorbed.	O consumed.	CO_2 expired.	O absorbed.	O consumed.
236 c.c.	45·1 c.c.	282 c.c.	225 c.c.	32·2 c.c.	257 c.c.
229 „	59·8 „	289 „	212 „	34·2 „	246 „
222 „	46·8 „	269* „	211 „	35·8 „	247 „

We now have to account for the fourth person under experiment giving results at variance with the others. In this case, instead of the CO_2 produced and O absorbed varying in opposite directions as they do with the three other persons experimented upon, they rise and fall together, their ratios being somewhat similar. This will be seen in the following table, in which each figure is the mean of two determinations nearest to each other.

B. F. Davis under Experiment. Under the Influence of Food.

	Winter.			Summer.		
	CO_2 expired.	O absorbed.	Ratio.	CO_2 expired.	O absorbed.	Ratio.
	c.c.	c.c.		c.c.	c.c.	
	264·1	59·9	0·227	252·5	44·3	0·175
	255·5	59·9	0·234	247·0	50·1	0·203
	236·0	50·5	0·214	235·3	44·8	0·190
	219·8	49·6	0·226	210·6	37·4	0·177
Means..	234·8	55·0	0·225	236·3	44·1	0·186

* The figures marked with an asterisk show the only exceptions.

These experiments show a remarkable uniformity of results in winter and summer respectively.

As to the experiments fasting, not tabulated in this paper, while in summer they agree with those of the three other persons under experiment, no uniform relation can be traced in winter between the CO_2 expired and O absorbed.

In this case, in the winter and summer experiments referred to, the CO_2 expired and O absorbed may be said to rise and fall together, which means that, instead of a tendency to a constant figure for O consumed, there is, at all events between 1 and 2 hrs. after a full meal, the reverse tendency, and the figure for O consumed is observed either to rise or fall. Consequently there must be in the case of Mr. Davis a function of the body different from the corresponding function in the three other persons. Now this gentleman is just twenty-one years of age and is still growing; indeed, there has been a marked appearance in him of physical development within the last few months, and I fully believe that this is the cause of the present result. While with the three other persons as the carbonic acid increased the oxygen absorbed diminished, in this particular case as the CO_2 increases the O absorbed also becomes larger, this excess being due, it may be concluded, to the requirements of new tissue.

Influence of Exercise.

The method of investigation was as follows:—In order to adopt a kind of muscular exercise similar, as near as possible, to one in common use, I selected the very simple act of stepping, within a small area, which imitated walking. This was done by raising the feet alternately sixty-eight times a minute, according to the striking of a metronome. I raised the feet by nearly 10 cm., and Mr. Davis by nearly 18 cm., measured at the heel; consequently the degree of exercise was not the same in each case. Moreover, the exercise may not have been strictly regular in every experiment, although sufficiently so for the purpose in view. In most cases, before exercise was taken, a preliminary experiment was made on the person *in repose;* with that object he rested for half an hour, reclining in a deck chair, then the air expired in a recorded time (in absolutely natural breathing) was collected in the two bell-jars, measured, and then transferred to an india-rubber bag faced with oil silk, in order to liberate the bell-jars for further use. The next part of the experiment was stepping and breathing the air expired into a bell-jar during exercise. It must be understood that this air was not collected during the whole time the exercise lasted, but only at the end of that time, and during about "3 mins."

After the air expired, under exercise, had been collected in a bell-jar, the person experimented upon sat down in the deck chair without

breathing, and then the air he expired was collected as usual for a period of 13 or 14 mins. In this way the whole phenomenon of respiration under exercise could be closely followed. The CO_2 alone, or CO_2 and O, were determined in the air expired in the state of repose, the CO_2 and O were determined in the air expired under exercise, and the CO_2 alone in the air expired while resting after the period of exercise. It had been found, experimentally, that the time taken to fill the two bell-jars (13 to 14 mins.) was sufficient for the CO_2 expired to return to its normal amount.

The object of the experiment was as follows:—The first stage in repose afforded data towards the comparison of the effects of exercise on the respiratory functions with those functions in the state of rest. The second stage had for its object to determine the CO_2 and O expired in a given time while under the exercise. The third stage showed the amount of CO_2 given out, while quite still, after the exercise had been concluded; and the excess of CO_2 thus obtained over the amount of CO_2 which would have been expired in the same lapse of time in perfect repose was looked upon as CO_2 which had accumulated in the blood during exercise, and this was proved by subsequent experiment.

[18*th August*.—Without entering at present into a discussion of this subject, a number of experiments, which I regret space does not allow me to describe, have shown most distinctly that respiration while in repose following exercise cannot be compared to forced respiration, inasmuch as in forced respiration the excess of CO_2 expired is much less than after exercise; and, moreover, immediately after a return to natural breathing after forced respiration the CO_2 expired is diminished nearly, although not quite, to the same amount as it had been increased under forced breathing, and this is not observed as a sequel to exercise.

I feel called upon to make this remark in due consideration to C. Speck's interesting paper on the consumption of oxygen and production of carbonic acid ('Schriften der Gesellschaft zur Beförderung der Gesammten Naturwissenschaften zu Marburg,' vol. 10, 1871.)]

The experiments on myself will be considered first: They were undertaken in December, January, and February last, and, with two exceptions, all between 1 and 2 hrs. after a full luncheon. The duration of the exercise was from about 17 to 19 mins., this period being selected, because after 19 or 20 mins. the phenomena were found to lose their regularity. The respiratory changes were also observed to be more regular in winter than in summer, and especially under the direct influence of digestion; or under circumstances producing most carbonic acid in the state of repose.

These experiments show that, in my case, under the kind of exercise taken, the amount of CO_2 expired per minute was very

Table showing Results of Experiments under Exercise.

The Author.

Perfect state of repose.	Winter experiments, 1—2 hours after meal.					Resting after exercise.		
CO_2 expired.	Duration of exercise.		CO_2 expired per minute.	O absorbed per minute.	Excess CO_2 expired in exercise over CO_2 in repose.	Total CO_2 retained.	CO_2 absorbed per minute.	Ratio of excess CO_2 expired under exercise to CO_2 absorbed.
c.c.	mins.	secs.	c.c.	c.c.	c.c.	c.c.	c.c.	
242·0	19	4	450·9	—	208·9	519	27·2	0·130
193·7	17	42	484·4	—	290·7	719	40·6	0·140
234·3	18	11	402·0	—	167·7	334	18·4	0·110
227·2	19	12	421·7	—	198·5	479	24·9	0·126
235·8	18	56	436·8	—	201·0	430	22·7	0·113
Fasting { 190·1	18	34	402·6	60·9	212·5	501	27·0	0·127
Fasting { 192·3	18	36	482·0	60·6	289·7	606	36·5	0·126
Fasting { 165·6	18	23	352·0	72·5	186·4	418	22·8	0·122
Fasting { 232·3	17	40	472·1	67·5	240·0	500	28·3	0·118
220·0	18	43	436 8	—	216·8	490	25·9	0·119
213·3	18	31	434·1	65·4	221·2	500	27·4	0·123

nearly twice the amount expired in a perfect state of repose (213·3 c.c. and 434·1 c.c.); the mean excess, 221·7, may be looked upon as due to the production of heat to be converted into motion, but it will be seen presently that there is really more heat developed from carbon burnt to the extent of about 12 per cent. of the above excess. The oxygen absorbed, 65·4 c.c., looks much too high, as the mean amount obtained under the influence of food in my last experiments is 35·7, and at first it occurred to me that more oxygen was really absorbed under exercise than in repose. But on inquiring closely into the present result, it became obvious that this figure for oxygen absorbed included some oxygen retained in the body as CO_2. The next point was to determine, if possible, how much oxygen was absorbed for tissue-change, and how much was retained as CO_2. This result was obtained by a consideration of the third stage of the experiment which concerned resting after exercise. In this third stage the mean excess of CO_2 found to have been expired under exercise over the CO_2 expired in repose is equal to 500 c.c. for a mean exercise of 18 mins. 31 secs.; or, in other words, in 18 mins. 31 secs. an amount of CO_2 had accumulated in the body equal to 500 c.c. Assuming that this accumulation took place regularly, it would have amounted to a mean of 27·6 c.c. per minute. We are now in a position to find out the volume of CO_2 present, together with the amount of O absorbed per minute; this is done by subtracting the volume of CO_2 absorbed per minute, or 27·4 from the volume of O entered as absorbed, or 65·4; this gives 38 c.c. for the actual volume of oxygen absorbed, which is very near to the figure 35·7, the mean volume of oxygen absorbed in my case under food and in the state of repose.

It then occurred to me that the total CO_2 retained in the blood under exercise might bear some proportion to the excess of CO_2 expired under exercise over the CO_2 expired sitting. On calculating these relations I found that there certainly was such a ratio. Of course the ratio varied somewhat in each experiment, but the means of the ten experiments gave the figure 0·123, while the extremes were 0·110 and 0·140. Therefore, by multiplying the mean ratio 0·123 by the excess of the CO_2 under exercise over the CO_2 in repose, the result will give, with a certain degree of approximation, the figure for the CO_2 absorbed per minute during the exercise without being at the trouble of determining this figure experimentally.

We now turn to the corresponding experiments made on Mr. Davis. There are five of them; the sixth was discarded from some irregularity which could not be accounted for.

In the first experiment the air expired was collected on beginning the exercise; in the second it was collected six minutes after exercise was begun; in the third 9 mins. after; in the fourth 12 mins.; and in the fifth 15 mins. after.

Results of Experiments under Exercise.

B. F. Davis, in Winter, 1—2 hrs. after a Meal.

Duration of exercise before collecting air....	0 min.	6 mins.	9 mins.	12 mins.	15 mins.
	c.c.	c.c.	c.c.	c.c.	c.c.
O consumed...........	719·0	770·0	834·0	799·0	775·0
CO_2 produced.........	634·0	659·7	743·0	682·6	660·0
O and CO_2 absorbed....	84·8	110·3	90·0	116·3	115·1

The figures obtained under the heading "O and CO_2 absorbed," were, with the exception perhaps of the first, fairly close to each other, with a mean of 103 c.c. By multiplying the factor 0·123 found in my experiment by the mean excess of CO_2 under exercise over CO_2 in repose (the data in repose being obtained from other experiments), and subtracting the result from the O and CO_2 found as absorbed, as was done in the other case, the figure for carbonic acid absorbed is 50, and for O absorbed 53; the latter being very near "55," which is, for Mr. Davis, the mean volume of oxygen absorbed after a meal in the state of repose. Therefore, again, in the present case in the winter experiments, between 1 and 2 hrs. after a full meal, by applying the factor 0·123, we find the same volume of oxygen absorbed under exercise as when in a perfect state of repose. In the experiments on Mr. Davis, the third stage had been omitted, as the experiments were done before any attempt to judge experimentally of the CO_2 absorbed in the blood had been thought of.

So far the results of these experiments must be considered as depending to a great extent on the season of the year—winter—on the time which has elapsed since food has been partaken of—1—2 hrs.—and on the period during which exercise has been taken—a period not exceeding 18 or 19 mins. It stands to reason that after exercise has lasted a certain time the blood cannot take up any more CO_2, and this time does not appear to exceed 19 or 20 mins. It was found, experimentally, that after that period the volume of CO_2 accumulated in the blood varies greatly.

The exercise was continued in other experiments for 33 mins., 42 mins., and 1½ hrs., when the volume of CO_2 stored up in the blood was found to range from 0 c.c. to 353 c.c. In the warm summer weather the CO_2 formed under exercise appears to leave the blood at the lungs more rapidly than in winter. So far I have not succeeded in finding any uniform volume absorbed during any given time in spring and summer, although a number of experiments were carefully made with that object in view.

Therefore, so far, the accumulation of CO_2 in the body may only be considered as regular under certain fixed conditions; but there is invariably a tendency towards CO_2 being retained in the blood the first few minutes exercise is taken after a period of repose.

A certain number of minutes after exercise has been commenced the CO_2 stored up in the blood is given out. In C. Bernard's experiment, where a solution of sulphuretted hydrogen is injected into the circulation of a dog, the gas comes out at the mouth in large volumes in a few successive expirations following deep inspirations. I should be inclined to think that a similar phenomenon takes place with reference to CO_2 in respiration under exercise, the gas accumulates in the blood up to a certain stage, and is then given out in the form of a wave, after which the accumulation goes on afresh, but the phenomenon is not regular, and depends on many causes which would be very difficult to determine. My experiments are certainly opposed to the idea that under exercise the CO_2 is eliminated as fast as it is produced, leaving a uniform balance of CO_2 in the blood. With prolonged exercise and training the intermittence would probably become less and less.

One of the questions for investigation which occurred to me in the course of this enquiry was the time required for the carbonic acid expired and oxygen absorbed to return to their normal condition of rest after the stepping exercise. This portion of work was done in the season 1891–92, when Mr. Darnell Smith, B.Sc., acted as my assistant. We both submitted to experiment. The first stage was the determination of CO_2 expired and O absorbed after resting for half an hour perfectly still in the deck chair.* Then the person under experiment took the stepping exercise for a quarter of an hour, and sat down, remaining quite still, for 10 mins. After that lapse of time the CO_2 expired and O absorbed were again determined. The results are shown in the following table (p. 51).

It will be seen in this table that, with both of us, after 10 mins.' rest the CO_2 expired had returned to the normal, or very nearly so. In my case the CO_2 had quite recovered its mean in repose; in Mr. Smith's it was only by 2 per cent. in excess. The oxygen absorbed was, however, quite altered from its original figures. Instead of 31·7 c.c. I absorbed per minute in repose, the figure had fallen to 24·8 c.c., and with Mr. Smith it had been reduced from 25·8 c.c. in repose to 14 c.c. The reason of this phenomenon appears to me very obvious. While the respiratory phenomena are all excited under exercise, the blood becomes charged with more oxygen than it can

* In three of these experiments, on the assumption that 30 mins.' rest after exercise were sufficient for a return of normal breathing in repose, the CO_2 and O were determined 30 mins. after the exercise was over instead of before the exercise was begun.

Time for Return of Normal Respiration after Exercise.

The Author under experiment.

	CO_2 produced per minute.		O absorbed per minute.	
	After a rest of 30 min.	After 10 mins.' rest following exercise.	After 30 mins.' rest.	After 10 mins.' rest following exercise.
1........	222 c.c.	234 c.c.	37·1 c.c.	31·0 c.c.
2........	218 "	211 "	27·8 "	14·0 "
3........	195 "	185 "	34·6 "	22·0 "
4........	190 "	188 "	27·5 "	22·3 "
Means...	206 c.c.	204 c.c.	31·7 c.c.	24·8 c.c.
		D. Smith under experiment.		
1........	241 c.c.	232 c.c.	23·1 c.c.	7·3 c.c.
2........	271 "	239 "	29·0 "	10·7 "
3........	233 "	266 "	24·1 "	8·1 "
4........	249 "	222 "	35·1 "	32·1 "
5........	223 "	225 "	19·7 "	16·7 "
6........	208 "	213 "	23·5 "	9·1 "
Means...	228 c.c.	233 c.c.	25·8 c.c.	14·0 c.c.

hold in the state of rest; therefore under the state of repose following exercise, the blood is sufficiently rich in oxygen to supply the tissue-change without drawing upon atmospheric air for a further amount of the gas.

In the experiments related above, 10 mins.' rest after exercise sufficed for the carbonic acid expired to return to its normal amount in the state in repose; possibly with some other persons a little longer, say, 15 mins., may be necessary. As for the period of rest required for the O absorbed to return to its normal figure in repose, after stepping exercise, half an hour appeared perhaps barely sufficient. I have considered, however, half an hour's rest as long enough when active exercise had not been previously taken; and half an hour's repose was adopted, as a rule, to bring the body into the physiological state of rest.

The following is a summary of the contents of this paper:—

1st. I have shown that in three persons out of four there was a great tendency to an uniformity of figure for the oxygen *consumed* under similar physical circumstances (food, temperature, &c.), so that, if the CO_2 expired fell, the oxygen absorbed rose, and *vice versâ;* this was accounted for by assuming that an increase of CO_2

in the blood in the state of repose is produced at the expense of the O absorbed. The fourth person experimented upon exhibited no such tendency, the CO_2 expired and O absorbed rose and fell together, which was ascribed to the fact that he was still growing.

2nd. Experiments were made on the influence of exercise on respiration which showed that if stepping exercise is taken after a period of rest, there occurs for a few minutes an accumulation of CO_2 in the blood; of course the degree of this storage of CO_2 must be controlled by the normal amount of CO_2 produced in repose, and the kind of exercise taken; this storage would in the cold winter weather, and between 1 and 2 hrs. after food continue for about 18 or 20 mins. In my case the volume of CO_2 retained in the blood amounted to a mean of 500 c.c. while stepping sixty-eight times per minute about 10 cm. high. The CO_2 in store is next given out in the form of a wave, which is renewed after a certain lapse of time, so that there does not appear to be in respiration under exercise a fixed relation between the CO_2 expired and the CO_2 left in the blood. With practice and training this relation would probably become more and more uniform.

The storage of CO_2 in winter and after food was found to exhibit a certain relation to the excess of CO_2 expired under exercise over the CO_2 expired in repose; but 18 or 20 mins. after exercise had been commenced this relation failed to show itself any longer.

The ratio in question was the same with two different persons; but further experiment is required to determine whether it can be looked upon as general; the mean ratio found is shown by the figure 0·123; therefore, so far as the present enquiry goes, and under the conditions expressed in this paper, by multiplying this figure 0·123 by the excess of CO_2 given out per minute under exercise over the CO_2 expired in repose during the same lapse of time, the result will show the volume of CO_2 absorbed in the blood per minute.

After the exercise had lasted 18 to 20 mins., the volume of CO_2 stored up in the blood after exercise was found to vary.

3rd. After the exercise adopted in this enquiry had been followed by a complete repose of 10 mins., the CO_2 expired had returned to the normal in repose, but the volume of O absorbed per minute had considerably fallen, clearly owing to the blood having charged itself with oxygen during exercise, so that the first few minutes after rest was taken the blood was in a condition to supply oxygen for tissue-changes without taking it from the air breathed. After half an hour's perfect rest following exercise the respiratory changes had returned to their normal state of repose, or nearly so, the oxygen absorbed still occasionally showing signs of being a little lower than before exercise had been taken.

VI. "The Glucoside Constitution of Proteid Matter." By F. W. Pavy, M.D., F.R.S. Received May 20, 1893.

(Abstract.)

For a long time I have adopted a process for separating the glycogen of the liver consisting in boiling with potash, pouring into alcohol, and collecting the precipitate. For the purpose of estimation, the precipitated glycogen was converted by means of sulphuric acid into glucose, the determination of which gave the information required. I afterwards applied the process to blood, and the various organs and tissues of the body, with the result that a more or less notable amount of cupric oxide reducing product was obtained, which I at the time looked upon as taking origin, as in the case of the liver, from glycogen.

When small quantities were operated upon for quantitative analysis, I experienced no difficulty in obtaining the product looked for. When large quantities, however, were taken for the purpose of collecting it and studying its properties, I invariably failed to obtain anything like the amount that ought to have been yielded according to the indications afforded by the quantitative analysis conducted. These attempts were undertaken at different times upon blood, eggs, and the spleen. It was obvious there was something connected with the extraction with which I was not acquainted, and it was through prosecuting inquiry to clear up the discrepancy that I came across the knowledge revealed in this communication.

Detailed particulars are given in the communication of experiments with various animal and vegetable proteid matters, showing that the amount of cupric oxide reducing product obtained after the application of the potash process varied through the instrumentality of the potash. If free glycogen or starch had been the source of it, the treatment with potash should have produced no effect beyond dissolving the associated nitrogenous matter and placing it in a position to be separable by the agency of the alcohol, and no difference should have resulted from varying the strength of the alkali or the length of time of contact. The results obtained led to the conclusion that the cupric oxide reducing product must have taken origin from some other source than free glycogen or starch, and pointed to its having been derived from a cleavage of the proteid molecule.

The steps of procedure employed for obtaining the product susceptible of conversion into a cupric oxide reducing body (sugar) from proteid matter are described at length, and an account given of the difficulties which had led to a want of success in my early endeavours to collect the body in quantity. With the knowledge

that is now possessed, no difficulty is experienced in obtaining it in any amount that may be required for the purpose of examination or experiment. Coagulated egg-albumin, purified by water, was selected as the most suitable representative form of proteid to serve for yielding the product.

The properties of the material obtained are as follows :—

In the dried state it forms a hard glassy resinoid mass.

It is readily soluble in water, giving a clear solution.

It yields no coloration with iodine.

It possesses no CuO reducing power.

Strong alcohol precipitates it. Spirit of 85 to 90 per cent. strength precipitates it in great part, if not completely, as an adherent tenacious gummy mass, from which the alcohol may be decanted off, and which may be afterwards worked up to form a sticky material. Absolute alcohol, used freely, throws it down as a fine white precipitate with no tendency to coalesce to a gummy mass. Upon the gummy precipitate absolute alcohol exerts a dehydrating action, causing it to harden and assume a crumbled, in place of a cohesive, state. With spirit below 85 per cent. strength, precipitation becomes more and more incomplete, and the precipitate produced by the weaker kind of spirit assumes a loose or non-adherent form. The incomplete precipitation with weak strengths is readily betrayed by the further precipitation which occurs upon adding more alcohol to the supernatant spirit.

In its physical characters, it presents a resemblance to Landwehr's "animal gum." Chemically, it also resembles Landwehr's "animal gum," in forming a copper compound on being treated with cupric sulphate and caustic potash, from which it is susceptible of recovery by the agency of nitric acid and subsequent precipitation with alcohol.

I have applied Landwehr's process to the product under consideration derived from the action of potash upon albumin, and have found that it behaves throughout like Landwehr's "animal gum," and similarly yields a CuO reducing body. Moreover, I have found that this CuO reducing body gives, with phenylhydrazine, needle crystals of glucosazone, of which I have obtained micro-photographs.

The product, it may be finally remarked, possesses the property of diffusibility, a character in which it differs from the amylose carbohydrates—starch, glycogen, dextrin. I have been unable to find any statement about the diffusibility or otherwise of "animal gum," but it is described as being a constant constituent of urine, which may be regarded as indicative of its being of a diffusible nature.

The CuO *Reducing Product*.—By the action of mineral acids, the first-formed product, of which I have been speaking, undergoes con-

version into a CuO reducing material. For effecting this conversion I have for years past been in the habit of using 2 per cent. sulphuric acid. As I have said, my impression originally was that the product consisted of glycogen, and from observations upon the conversion of starch and glycogen into glucose, I had formed the opinion that sulphuric acid of the strength named best met the requirements. Boiling for about an hour and a half with an inverted condenser was resorted to, or the autoclave was used, half-an-hour's exposure to a temperature of 150° C. producing an equivalent effect, the results given by the two methods being practically identical. Where the object has been a quantitative analysis, the acid has been subsequently neutralised with potash, the sulphate formed not occasioning any inconvenience; but should it be desired to collect the reducing product, the acid must be removed by precipitation, best effected by the agency of barium carbonate; the filtrate, after evaporation to dryness on the water-bath, yields the material in the state desired.

I had been led by certain circumstances to consider that the reducing product derived from the action of the 2 per cent. acid did not consist of glucose, and had unsuccessfully tried, by more extended periods of boiling, to get it carried further. Subsequently I renewed my efforts in this direction. In experimenting with cellulose, I had found that but very slight action was produced by 2 per cent. acid, while considerable effect followed the use of acid of 10 per cent. strength, and it occurred to me that the body I was dealing with might behave similarly. Trying 10 per cent. acid, I found that the reducing power of the product became very nearly doubled, being raised in the proportion of from between 50 and 60 to 100. I next tried 50 per cent. acid, allowed to remain in contact with the product for one to three days, then diluted to 20 per cent., and boiled. The result stood about the same as after direct boiling with 10 per cent. acid. After 15 per cent. acid, also, a like result was obtained. As yet I have failed to carry the body to a higher stage of CuO reducing power than that produced by 10 per cent. acid, but I am still led to think that the glucose stage has not yet been reached. The effect of raising the reducing power is to give a semblance of a corresponding increase of material.

The following are the characters of the product obtained after the separation of the sulphuric acid by barium carbonate and evaporation of the filtered liquid to dryness:—

It presents the appearance of a sugary extractive, and has a pronounced baked-sugar odour.

It is very soluble in water, only slightly so in absolute alcohol, but considerably so in spirit of about 90 per cent. strength.

It is readily diffusible.

Boiled with caustic potash (Moore's test), the solution darkens.

It dissolves hydrated copper oxide in presence of excess of potash (Trommer's test), without producing a biuret reaction; at least, specimens are often procurable of which this can be said.

With Fehling's solution, it gives a strong and neat reaction, the reduced copper oxide falling as a dense red precipitate.

Heated on the water-bath for two or three hours with phenylhydrazine and acetic acid, it yields, on cooling, a crystalline osazone in the form of needles clustered into sheaves or brushes, or radiating in dense round masses. Often the composite character of these round masses is only to be seen by close inspection of the circumference. Whilst a certain type prevails, variations within limits are noticeable, and sometimes an approach to a spike constitution is observable. The crystals are soluble in alcohol, from which they may be recrystallised.

It gives with benzoyl chloride an insoluble compound, in accord with the behaviour of carbohydrates.

With α-naphthol and excess of strong sulphuric acid, it behaves like sugar in giving a deep-violet colour, and leading on dilution to the formation of a violet-blue precipitate soluble in alcohol, ether, and caustic potash, with the production of yellow solutions, but insoluble in hydrochloric acid, a character by which, according to Molisch, the precipitate produced from sugar is distinguishable from that derived from peptone and various albuminous bodies.

With thymol and excess of strong sulphuric acid, it again behaves like sugar, giving a deep-red coloration, followed, on dilution, by the production of a carmine-red precipitate, soluble in alcohol, ether, and caustic potash, with formation of pale-yellow solutions, and in ammonia with formation of a bright-yellow solution. The precipitate, as in the case of the α-naphthol test, is found to possess the character of insolubility in hydrochloric acid.

It is susceptible of being thrown down in combination with oxide of lead, by the employment of lead acetate and ammonia, and is afterwards recoverable from the compound. From the sugar thus recovered, the usual reaction is obtainable with Fehling's solution (provided the ammonia has been fully removed); and also the crystalline osazone with phenylhydrazine, of which micro-photographs are furnished.

From this assemblage of positive characters, it appears to me there can be no doubt that the CuO reducing body obtainable from proteid matter consists of sugar. I should state, however, that I have not yet obtained it in a form to prove fermentable; and Dr. Sheridan Lea, who kindly examined it with the polarimeter, failed to notice any rotation of which he could speak with certainty. He adds there was, if anything, a tendency to lævorotation, but not amounting to more than 0·1°.

In addition to the evidence derivable from the positive characters mentioned as possessed by the CuO reducing product, there is the corroborative evidence to be taken into account supplied by the characters of agreement observable between my primary non-reducing product and Landwehr's "animal gum." As already shown, the product in question can be thrown down, precisely like "animal gum," in combination with copper oxide, and is afterwards recoverable and convertible into a CuO reducing body, which yields with phenylhydrazine a crystalline osazone. The micro-photos. supplied show two kinds of crystals, one consisting distinctly of the long needles of glucosazone. In its ready mode of separating out, the osazone was observed to agree with that derived from glucose. A 10 per cent. strength of sulphuric acid was employed in obtaining the CuO reducing product operated upon.

Presents, June 8, 1893.

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June 15, 1893.

The LORD KELVIN, D.C.L., LL.D., President, in the Chair.

A List of the Presents received was laid on the table, and thanks ordered for them.

Professor William Tennant Gairdner and Professor James William H. Trail were admitted into the Society.

The following Papers were read:—

I. "On the Elasticity of a Crystal according to Boscovich." By the LORD KELVIN, P.R.S. Received June 8, 1893.

§ 1. A crystal in nature is essentially a homogeneous assemblage of equal and similar molecules, which for brevity I shall call crystalline molecules. The crystalline molecule may be the smallest portion which can be taken from the substance without chemical decomposition, that is to say, it may be the group of atoms kept together by chemical affinity, which constitutes what for brevity I shall call the chemical molecule; or it may be a group of two, three, or more of these chemical molecules kept together by cohesive force. In a crystal of tartaric acid the crystalline molecule may be, and it seems to me probably is, the chemical molecule, because if a crystal of tartaric acid is dissolved and recrystallised it always remains dextro-chiral. In a crystal of chlorate of soda, as has been pointed out to me by Sir George Stokes, the crystalline molecule probably consists of a group of two or more of the chemical molecules constituting chlorate of soda, because, as found by Marbach,* crystals of the substance are some of them dextro-chiral and some of them levo-chiral; and if a crystal of either chirality is dissolved the solution shows no chirality in its action on polarised light; but if it is recrystallised the crystals are found to be some of them dextro-chiral and some of them levo-chiral, as shown both by their crystalline forms and by their action on polarised light. It is possible, however, that even in chlorate of soda the crystalline molecule may be the chemical molecule, because it may be that the chemical molecule in solution has its atoms relatively mobile enough not to remain persistently in any dextro-chiral or levo-chiral grouping, and that each individual chemical molecule settles

* 'Pogg. Ann.,' vol. 91, pp. 482—487 (1854); or 'Ann. de Chimie,' vol. 43 (55), pp. 252—255.

into either a dextro-chiral or levo-chiral configuration in the act of forming a crystal.

§ 2. Certain it is that the crystalline molecule has a chiral configuration in every crystal which shows chirality in its crystalline form or which produces right- or left-handed rotation of the plane of polarisation of light passing through it. The magnetic rotation has neither right-handed nor left-handed quality (that is to say, no chirality). This was perfectly understood by Faraday and made clear in his writings, yet even to the present day we frequently find the chiral rotation and the magnetic rotation of the plane of polarised light classed together in a manner against which Faraday's original description of his discovery of the magnetic polarisation contains ample warning.

§ 3. These questions, however, of chirality and magnetic rotation do not belong to my present subject, which is merely the forcive* required to keep a crystal homogeneously strained to any infinitesimal extent from the condition in which it rests when no force acts upon it from without. In the elements of the mathematical theory of elasticity† we find that this forcive constitutes what is called a homogeneous stress, and is specified completely by six generalised force-components, $p_1, p_2, p_3, \ldots, p_6$, which are related to six corresponding generalised components of strain, $s_1, s_2, s_3, \ldots, s_6$, by the following formulas:—

$$w = \tfrac{1}{2}(p_1s_1+p_2s_2+\ldots+p_6s_6) \quad \ldots\ldots\ldots\ldots (1),$$

where w denotes the work required per unit volume to alter any portion of the crystal from its natural unstressed and unstrained condition to any condition of infinitesimal homogeneous stress or strain:

$$p_1 = \frac{dw}{ds_1}, \ldots, p_6 = \frac{dw}{ds_6} \quad \ldots\ldots\ldots\ldots (2),$$

where $\frac{d}{ds_1}, \ldots, \frac{d}{ds_6}$ denote differential coefficients on the supposition that w is expressed as a homogeneous quadratic function of $s_1, \ldots, s_6$:

$$s_1 = \frac{\partial w}{dp_1}, \ldots, s_6 = \frac{\partial w}{dp_6} \quad \ldots\ldots\ldots\ldots (3),$$

where $\frac{\partial}{dp_1}, \ldots, \frac{\partial}{dp_6}$ denote differential coefficients on the supposition that w is expressed as a homogeneous quadratic function of $p_1, \ldots, p_6$.

* This is a word introduced by my brother, the late Professor James Thomson, to designate any system of forces.

† 'Phil. Trans.,' April 24, 1856, reprinted in vol. iii, 'Math. and Phys. Papers' (Sir W. Thomson), pp. 84—112.

§ 4. Each crystalline molecule in reality certainly experiences forcive from some of its nearest neighbours on two sides, and probably also from next nearest neighbours and others. Whatever the mutual forcive between two mutually acting crystalline molecules is in reality, and however it is produced, whether by continuous pressure in some medium, or by action at a distance, we may ideally reduce it, according to elementary statical principles, to two forces, or to one single force and a couple in a plane perpendicular to that force. Boscovich's theory, a purely mathematical idealism, makes each crystalline molecule a single point, or a group of points, and assumes that there is a mutual force between each point of one crystalline molecule and each point of neighbouring crystalline molecules, in the line joining the two points. The very simplest Boscovichian idea of a crystal is a homogeneous group of single points. The next simplest idea is a homogeneous group of double points.

§ 5. In the present communication, I demonstrate that, if we take the very simplest Boscovichian idea of a crystal, a homogeneous group of single points, we find essentially six relations between the twenty-one coefficients in the quadratic function expressing w, whether in terms of $s_1, \ldots, s_6$ or of $p_1, \ldots, p_6$. These six relations are such that infinite resistance to change of bulk involves infinite rigidity. In the particular case of an equilateral* homogeneous assemblage with such a law of force as to give equal rigidities for all directions of shearing, these six relations give $3k = 5n$, which is the relation found by Navier and Poisson in their Boscovichian theory for isotropic elasticity in a solid. This relation was shown by Stokes to be violated by many real homogeneous isotropic substances, such, for example, as jelly and india-rubber, which oppose so great resistance to compression and so small resistance to change of shape, that we may, with but little practical error, consider them as incompressible elastic solids.

§ 6. I next demonstrate that if we take the next simplest Boscovichian idea for a crystal, a homogeneous group of double points, we can assign very simple laws of variation of the forces between the points which shall give any arbitrarily assigned value to each of the twenty-one coefficients in either of the quadratic expressions for w.

§ 7. I consider particularly the problem of assigning such values to the twenty-one coefficients of either of the quadratic formulas as shall render the solid incompressible. This is most easily done by taking w as a quadratic function of $p_1, \ldots, p_6$, and by taking one of these

* That is to say, an assemblage in which the lines from any point to three neighbours nearest to it and to one another are inclined at 60° to one another; and these neighbours are at equal distances from it. This implies that each point has twelve equidistant nearest neighbours around it, and that any tetrahedron of four nearest neighbours has for its four faces four equal equilateral triangles.

generalised stress components, say p_6, as uniform positive or negative pressure in all directions. This makes s_6 uniform compression or extension in all directions, and makes $s_1, \ldots, s_5$ five distortional components with no change of bulk. The condition that the solid shall be incompressible is then simply that the coefficients of the six terms involving p_6 are each of them zero. Thus, the expression for w becomes merely a quadratic function of the five distortional stress-components, $p_1, \ldots, p_5$, with fifteen independent coefficients: and equations (3) of § 3 above express the five distortional components as linear functions of the five stress-components with these fifteen independent coefficients.

Added July 18, 1893.

§ 8. To demonstrate the propositions of § 5, let OX, OY, OZ be three mutually perpendicular lines through any point O of a homogeneous assemblage, and let x, y, z be the coordinates of any other point P of the assemblage, in its unstrained condition. As it is a homogeneous assemblage of single points that we are now considering, there must be another point P′, whose coordinates are $-x, -y, -z$. Let $(x+\delta x, y+\delta y, z+\delta z)$ be the coordinates of the altered position of P in any condition of infinitesimal strain, specified by the six symbols e, f, g, a, b, c, according to the notation of Thomson and Tait's 'Natural Philosophy,' Vol. I, Pt. II, § 669. In this notation, e, f, g denote simple infinitesimal elongations parallel to OX, OY, OZ respectively; and a, b, c infinitesimal changes from the right angles between three pairs of planes of the substance, which, in the unstrained condition, are parallel to (XOY, XOZ), (YOZ, YOX), (ZOX, ZOY) respectively (all angles being measured in terms of the radian). The definition of a, b, c may be given, in other words, as follows, with a taken as example: a denotes the difference of component motions parallel to OY of two planes of the substance at unit distance asunder, kept parallel to YOX during the displacement; or, which is the same thing, the difference of component motions parallel to OZ of two planes at unit distance asunder kept parallel to ZOX during the displacement. To avoid the unnecessary consideration of rotational displacement, we shall suppose the displacement corresponding to the strain-component a to consist of elongation perpendicular to OX in the plane through OX bisecting YOZ, and shrinkage perpendicular to OX in the plane through OX perpendicular to that bisecting plane. This displacement gives no contribution to δx, and contributes to δy and δz respectively $\frac{1}{2}az$ and $\frac{1}{2}ay$. Hence, and dealing similarly with b and c, and taking into account the contributions of e, f, g, we find

$$\left.\begin{aligned}\delta x &= ex+\tfrac{1}{2}(bz+cy)\\ \delta y &= fy+\tfrac{1}{2}(cx+az)\\ \delta z &= gz+\tfrac{1}{2}(ay+bx)\end{aligned}\right\} \quad \ldots\ldots\ldots\ldots \quad (4).$$

§ 9. In our dynamical treatment below, the following formulas, in which powers higher than squares or products of the infinitesimal ratios $\delta x/r$, $\delta y/r$, $\delta z/r$ (r denoting OP) are neglected, will be found useful.

$$\frac{\delta r}{r} = \frac{x\delta x+y\delta y+z\delta z}{r^2}+\tfrac{1}{2}\frac{\delta x^2+\delta y^2++\delta z^2}{r^2}-\tfrac{1}{2}\left(\frac{x\delta x+y\delta y+z\delta z}{r^2}\right)^2 \quad (5).$$

Now by (4) we have

$$x\delta x+y\delta y+z\delta z = ex^2+fy^2+gz^2+ayz+bzx+cxy \quad \ldots\ldots \quad (6),$$

and

$$\begin{aligned}\delta x^2+\delta y^2+\delta z^2 = e^2x^2+f^2y^2+g^2z^2 \\ +\tfrac{1}{4}[a^2(y^2+z^2)+b^2(z^2+x^2)+c^2(x^2+y^2)] \\ +[\tfrac{1}{2}bc+(f+g)a]yz+[\tfrac{1}{2}ca+(g+e)b]zx+[\tfrac{1}{2}ab+(e+f)c]xy \quad (7).\end{aligned}$$

Using (6) and (7) in (5), we find

$$\frac{\delta r}{r} = r^{-2}(ex^2+fy^2+gz^2+ayz+bzx+cxy)+Q(e,f,g,a,b,c) \ldots \quad (8),$$

where Q denotes a quadratic function of e, f, &c., with coefficients as follows :—

$$\left.\begin{array}{llll} \text{Coefficient of} & \tfrac{1}{2}e^2 & \text{is} & \dfrac{x^2}{r^2}-\dfrac{x^4}{r^4} \\ \quad ,, & \tfrac{1}{2}a^2 & ,, & \tfrac{1}{4}\dfrac{y^2+z^2}{r^2}-\dfrac{y^2z^2}{r^4} \\ \quad ,, & fg & ,, & -\dfrac{y^2z^2}{r^4} \\ \quad ,, & bc & ,, & \tfrac{1}{4}\dfrac{yz}{r^2}-\dfrac{x^2yz}{r^4} \\ \quad ,, & ea & ,, & -\dfrac{x^2yz}{r^4} \\ \quad ,, & eb & ,, & \tfrac{1}{2}\dfrac{zx}{r^2}-\dfrac{x^3z}{r^4} \end{array}\right\} \quad \ldots\ldots\ldots\ldots \quad (9),$$

and corresponding symmetrical expressions for the other fifteen coefficients.

§ 10. Going back now to § 3, let us find w, the work per unit volume, required to alter our homogeneous assemblage from its unstrained condition to the infinitesimally strained condition specified by e, f, g, a, b, c. Let $\phi(r)$ be the work required to bring two points of the system from an infinitely great distance asunder to distance r. This is what I shall call the mutual potential energy of two points at distance r. What I shall now call the potential energy of the whole system, and denote by W, is the total work which must be done to bring all the points of it from infinite mutual distances to their actual positions in the system; so that we have

$$W = \tfrac{1}{2}\Sigma\Sigma\phi(r) \qquad (10),$$

where $\Sigma\phi(r)$ denotes the sum of the values of $\phi(r)$ for the distances between any one point O, and all the others; and $\Sigma\Sigma\phi(r)$ denotes the sum of these sums with the point O taken successively at every point of the system. In this double summation $\phi(r)$ is taken twice over, whence the factor $\frac{1}{2}$ in the formula (10).

§ 11. Suppose now the law of force to be such that $\phi(r)$ vanishes for every value of r greater than $\nu\lambda$, where λ denotes the distance between any one point and its nearest neighbour, and ν any small or large numeric exceeding unity, and limited only by the condition that $\nu\lambda$ is very small in comparison with the linear dimensions of the whole assemblage. This, and the homogeneousness of our assemblage, imply that, except through a very thin surface layer of thickness $\nu\lambda$, exceedingly small in comparison with diameters of the assemblage, every point experiences the same set of balancing forces from neighbours as every other point, whether the system be in what we have called its unstrained condition or in any condition whatever of homogeneous strain. This strain is not of necessity an infinitely small strain, so far as concerns the proposition just stated, although in our mathematical work we limit ourselves to strains which are infinitely small.

§ 12. Remark also that if the whole system be given as a homogeneous assemblage of any specified description, and if all points in the surface-layer be held by externally applied forces in their positions as constituents of a finite homogeneous assemblage, the whole assemblage will be in equilibrium under the influence of mutual forces between the points; because the force exerted on any point O by any point P is balanced by the equal and opposite force exerted by the point P′ at equal distance on the opposite side of O.

§ 13. Neglecting now all points in the thin surface layer, let N denote the whole number of points in the homogeneous assemblage

within it. We have, in § 10, by reason of the homogeneousness of the assemblage,

$$\Sigma\Sigma\phi(r) = \mathrm{N}\Sigma\phi(r) \qquad \text{(11)},$$

and equation (10) becomes

$$\mathrm{W} = \tfrac{1}{2}\mathrm{N}\Sigma\phi(r) \qquad \text{(12)}.$$

Hence, by Taylor's theorem,

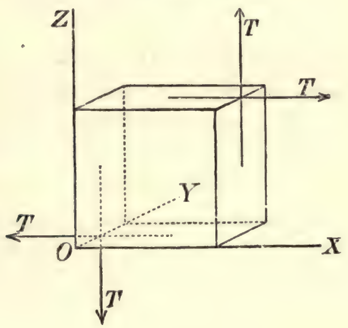
$$\delta\mathrm{W} = \tfrac{1}{2}\mathrm{N}\Sigma\{\phi'(r)\,\delta r + \tfrac{1}{2}\phi''(r)\,\delta r^2\} \qquad \text{(13)};$$

and using (8) in this, and remarking that if (as in § 14 below) we take the volume of our assemblage as unity, so that N is the number of points per unit volume, δW becomes the w of § 3; we find

$$w = \tfrac{1}{2}\mathrm{N}\Sigma\left\{\frac{\phi'(r)}{r}(ex^2+fy^2+gz^2+ayz+bzx+cxy)\right.$$
$$\left. + r\phi'(r)\,\mathrm{Q}(e,f,g,a,b,c) + \tfrac{1}{2}\frac{\phi''(r)}{r^2}(ex^2+fy^2+gz^2+ayz+bzx+cxy)^2\right\} \qquad \text{(14)}.$$

§ 14. Let us now suppose, for simplicity, the whole assemblage, in its unstrained condition, to be a cube of unit edge, and let P be the sum of the normal components of the extraneous forces applied to the points of the surface-layer in one of the faces of the cube. The equilibrium of the cube, as a whole, requires an equal and opposite normal component P in the opposite face of the cube. Similarly, let Q and R denote the sums of the normal components of extraneous force on the two other pairs of faces of the cube. Let T be the sum of tangential components, parallel to OZ, of the extraneous forces on either of the YZ faces. The equilibrium of the cube as a whole requires four such forces on the four faces parallel to OY, constituting

FIG. 1.



two balancing couples, as shown in the accompanying diagram. Similarly, we must have four balancing tangential forces S on the four faces parallel to OX, and four tangential forces U on the four faces parallel to OZ.

§ 15. Considering now an infinitely small change of strain in the cube from (e, f, g, a, b, c) to $(e+de, f+df, g+dg, a+da, b+db, c+dc)$; the work required to produce it, as we see by considering the definitions of the displacements e, f, g, a, b, c, explained above in § 8, is as follows,

$$dw = \mathrm{P}de + \mathrm{Q}df + \mathrm{R}dg + \mathrm{S}da + \mathrm{T}db + \mathrm{U}dc \quad \ldots\ldots \quad (15).$$

Hence we have

$$\left.\begin{array}{lll} \mathrm{P} = dw/de; & \mathrm{Q} = dw/df; & \mathrm{R} = dw/dg; \\ \mathrm{S} = dw/da; & \mathrm{T} = dw/db; & \mathrm{U} = dw/dc; \end{array}\right\} \quad (16).$$

Hence, by (14), and taking L, L to denote linear functions, we find

$$\left.\begin{array}{l} \mathrm{P} = \frac{1}{2}\mathrm{N}\Sigma\left\{\dfrac{\phi'(r)}{r}x^2 + \mathrm{L}(e, f, g, a, b, c)\right\} \\ \mathrm{S} = \frac{1}{2}\mathrm{N}\Sigma\left\{\dfrac{\phi'(r)}{r}yz + L(e, f, g, a, b, c)\right\} \end{array}\right\} \quad \ldots\ldots \quad (17),$$

and symmetrical expressions for Q, R, T, U.

§ 16. Let now our condition of zero strain be one* in which no extraneous force is required to prevent the assemblage from leaving it. We must have $\mathrm{P} = 0$, $\mathrm{Q} = 0$, $\mathrm{R} = 0$, $\mathrm{S} = 0$, $\mathrm{T} = 0$, $\mathrm{U} = 0$, when $e = 0$, $f = 0$, $g = 0$, $a = 0$, $b = 0$, $c = 0$. Hence, by (17), and the other four symmetrical formulæ, we see that

$$\left.\begin{array}{lll} \Sigma\dfrac{\phi'(r)}{r}x^2 = 0, & \Sigma\dfrac{\phi'(r)}{r}y^2 = 0, & \Sigma\dfrac{\phi'(r)}{r}z^2 = 0, \\ \Sigma\dfrac{\phi'(r)}{r}yz = 0, & \Sigma\dfrac{\phi'(r)}{r}zx = 0, & \Sigma\dfrac{\phi'(r)}{r}xy = 0 \end{array}\right\} \quad (18).$$

Hence, in the summation for all the points x, y, z, between which and the point O there is force, we see that the first term of the summed coefficients in Q, given by (9) above, vanishes in every case, except those of fg and ea, in each of which there is only a single term; and thus from (9) and (14) we find

* The consideration of the equilibrium of the thin surface layer, in these circumstances, under the influence of merely their proper mutual forces, is exceedingly interesting, both in its relation to Laplace's theory of capillary attraction, and to the physical condition of the faces of a crystal and of surfaces of irregular fracture. But it must be deferred.

$$\left.\begin{aligned} w = \tfrac{1}{2}\mathrm{N}\Big\{ & \tfrac{1}{2}e^2\Sigma\varpi\frac{x^4}{r^4}+(fg+\tfrac{1}{2}a^2)\,\Sigma\varpi\,\frac{y^2z^2}{r^4} \\ & +(bc+ea)\Sigma\varpi\,\frac{x^2yz}{r^4}+eb\Sigma\varpi\,\frac{x^3z}{r^4}+\&\text{c.}\Big\} \end{aligned}\right\} \quad \text{.. (19),}$$

where $$-r\phi'(r)+r^2\phi''(r) = \varpi \quad \ldots\ldots\ldots\ldots\ldots \text{(20).}$$

The terms given explicitly in (19) suffice to show by symmetry all the remaining terms represented by the "&c."

§ 17. Thus we see that with no limitation whatever to the number of neighbours acting with sensible force on any one point O, and with no simplifying assumption as to the law of force, we have in the quadratic for w equal values for the coefficients of fg and $\frac{1}{2}a^2$; ge and $\frac{1}{2}b^2$; ef and $\frac{1}{2}c^2$; bc and ea; ca and eb; and ab and ec. These equalities constitute the six relations promised for demonstration in § 5.

§ 18. In the particular case of an equilateral assemblage, with axes OX, OY, OZ parallel to the three pairs of opposite edges of a tetrahedron of four nearest neighbours, the coefficients which we have found for all the products except fg, ge, ef clearly vanish; because in the complete sum for a single homogeneous equilateral assemblage we have $\pm x$, $\pm y$, $\pm z$ in the symmetrical terms. Hence, and because for this case

$$\Sigma\varpi\frac{x^4}{r^4} = \Sigma\varpi\frac{y^4}{r^4} = \Sigma\varpi\frac{z^4}{r^4}, \quad \text{and} \quad \Sigma\varpi\frac{y^2z^2}{r^4} = \Sigma\varpi\frac{z^2x^2}{r^4} = \Sigma\varpi\frac{x^2y^2}{r^4} \quad \text{(21),}$$

(19) becomes

$$w = \tfrac{1}{2}\mathrm{A}\,(e^2+f^2+g^2)+\mathrm{B}\,(fg+ge+ef)+\tfrac{1}{2}n\,(a^2+b^2+c^2) \quad \text{.. (22),}$$

where $$\mathrm{A} = \tfrac{1}{2}\mathrm{N}\Sigma\varpi\frac{x^4}{r^4}, \quad \text{and} \quad \mathrm{B} = n = \tfrac{1}{2}\mathrm{N}\Sigma\varpi\frac{y^2z^2}{r^4} \quad \ldots\ldots \text{(23).}$$

§ 19. Looking to Thomson and Tait's 'Natural Philosophy,' § 695 (7),* we see that n in our present formula (22) denotes the rigidity-modulus relative to shearings parallel to the planes YOZ, ZOX, XOY; and that if we denote by n_1 the rigidity-modulus relative to shearing parallel to planes through OX, OY, OZ, and cutting (OY, OZ), (OZ, OX), (OX, OY) at angles of 45°, and if k denote the compressibility-modulus, we have

$$\left.\begin{aligned} \mathrm{A} = k+\tfrac{4}{3}n_1; \quad & \mathrm{B} = k-\tfrac{2}{3}n_1; \\ n_1 = \tfrac{1}{2}(\mathrm{A}-\mathrm{B}); \quad & k = \tfrac{1}{3}(\mathrm{A}+2\mathrm{B}) \end{aligned}\right\} \quad \ldots\ldots\ldots \text{(24);}$$

* This formula is given for the case of a body which is wholly isotropic in respect to elasticity moduluses; but from the investigation in §§ 681, 682 we see that our present formula, (22) or (25), expresses the elastic energy for the case of an elastic solid possessing cubic isotropy with unequal rigidities in respect to these two sets of shearings.

and our expression (22), for the elastic energy of the strained solid, becomes

$$2w = (k+\tfrac{4}{3}n_1)(e^2+f^2+g^2)+2(k-\tfrac{2}{3}n_1)(fg+ge+ef)+n(a^2+b^2+c^2) \quad \ldots\ldots (25)$$

§ 20. Using in (24) the equality $B = n$ shown in (23), we find

$$3k = 2n_1+3n \quad \ldots\ldots\ldots\ldots\ldots\ldots (26).$$

This remarkable relation between the two rigidities and the compressibility of an equilateral homogeneous assemblage of Boscovich atoms was announced without proof in § 27 of my paper on the "Molecular Constitution of Matter."* In it n denotes what I called the facial rigidity, being rigidity relative to shearings parallel to the faces of the principal cube:† and n_1 the diagonal rigidity, being rigidity relative to shearings parallel to any of the six diagonal planes through pairs of mutually remotest parallel edges of the same cube. By (24) and (23) we see that if the law of force be such that

$$\Sigma\varpi\frac{x^4}{r^4} = 3\Sigma\varpi\frac{y^2z^2}{r^4} \quad \ldots\ldots\ldots\ldots\ldots\ldots (27),$$

we have $n = n_1$, and the body constituted by the assemblage is wholly isotropic in its elastic quality. In this case (26) becomes $3k = 5n$, as found by Navier and Poisson; and thus we complete the demonstration of the statements of § 5 above.

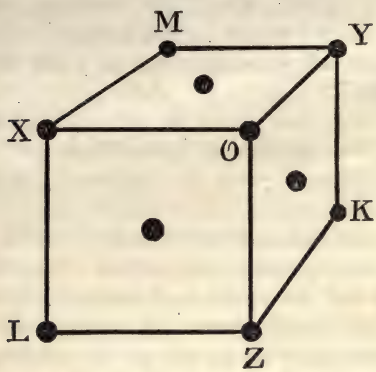
§ 21. A case which is not uninteresting in respect to Boscovichian theory, and which is very interesting indeed in respect to mechanical engineering (of which the relationship with Boscovich's theory has been pointed out and beautifully illustrated by M. Brillouin),‡ is the case of an equilateral homogeneous assemblage with forces only between each point and its twelve equidistant nearest neighbours. The annexed diagram (fig. 2) represents the point O and three of its twelve nearest neighbours (their distances λ), being in the middles of the near faces of the principal cube shown in the diagram; and three of its six next-nearest neighbours (their distances $\lambda\sqrt{2}$), being at X, Y, Z, the corners of the cube nearest to it; and, at other corners of the cube, three other neighbours K, L, M, which are next-next-next-nearest (their distances 2λ). The points in the middles of the three remote sides of the cube, not seen in the diagram, are next-next-nearest neighbours of O (their distances $\lambda\sqrt{3}$).

* 'R. S. E. Proc.,' July, 1889; Art. XCVII of my 'Math. and Phys. Papers,' vol. iii.

† That is to say, a cube whose edges are parallel to the three pairs of opposite edges of a tetrahedron of four nearest neighbours.

‡ 'Conférences Scientifiques et Allocutions' (Lord Kelvin), traduites et annotées; P. Lugol et M. Brillouin: Paris, 1893, pp. 320—325.

FIG. 2.



§ 22. Confining our attention now to O's nearest neighbours, we see that the nine not shown in the diagram are in the middles of squares obtained by producing the lines YO, ZO, XO to equal distances beyond O and completing the squares on all the pairs of lines so obtained. To see this more clearly, imagine eight equal cubes placed together, with faces in contact and each with one corner at O. The pairs of faces in contact are four squares in each of the three planes cutting one another at right angles through O; and the centres of these twelve squares are the twelve nearest neighbours of O. If we denote by λ the distance of each of them from O, we have for the coordinates x, y, z of these twelve points as follows:—

$$\left.\begin{array}{l}\left(0, \frac{\lambda}{\sqrt{2}}, \frac{\lambda}{\sqrt{2}}\right), \left(0, -\frac{\lambda}{\sqrt{2}}, \frac{\lambda}{\sqrt{2}}\right), \left(0, \frac{\lambda}{\sqrt{2}}, -\frac{\lambda}{\sqrt{2}}\right), \left(0, -\frac{\lambda}{\sqrt{2}}, -\frac{\lambda}{\sqrt{2}}\right) \\ \left(\frac{\lambda}{\sqrt{2}}, 0, \frac{\lambda}{\sqrt{2}}\right), \left(-\frac{\lambda}{\sqrt{2}}, 0, \frac{\lambda}{\sqrt{2}}\right), \left(\frac{\lambda}{\sqrt{2}}, 0, -\frac{\lambda}{\sqrt{2}}\right), \left(-\frac{\lambda}{\sqrt{2}}, 0, -\frac{\lambda}{\sqrt{2}}\right) \\ \left(\frac{\lambda}{\sqrt{2}}, \frac{\lambda}{\sqrt{2}}, 0\right), \left(-\frac{\lambda}{\sqrt{2}}, \frac{\lambda}{\sqrt{2}}, 0\right), \left(\frac{\lambda}{\sqrt{2}}, -\frac{\lambda}{\sqrt{2}}, 0\right), \left(-\frac{\lambda}{\sqrt{2}}, -\frac{\lambda}{\sqrt{2}}, 0\right)\end{array}\right\} \quad \ldots\ldots (28).$$

§ 23. Suppose now O to experience force only from its twelve nearest neighbours: the summations Σ of § 18 (23) will include just these twelve points with equal values of ϖ for all. These yield eight terms to $\Sigma(x^4/r^4)$, and four to $\Sigma(y^2z^2/r^4)$; and the value of each term in these sums is $\frac{1}{4}$. Thus we find that

$$A = N\varpi, \quad \text{and} \quad B = n = \tfrac{1}{2}N\varpi \ldots\ldots\ldots\ldots (29).$$

Hence and by (24), we see that

$$n_1 = \tfrac{1}{2}n \ldots\ldots\ldots\ldots\ldots\ldots\ldots\ldots (30).$$

Thus we have the remarkable result that, relatively to the principal cube, the diagonal rigidity is half the facial rigidity when each point experiences force only from its twelve nearest neighbours. This proposition was announced without proof in § (28) of "Molecular Constitution of Matter."*

§ 24. Suppose now the points in the middles of the faces of the cubes which in the equilateral assemblage are O's twelve equidistant nearest neighbours to be removed, and the assemblage to consist of points in simplest cubic order; that is to say, of Boscovichian points at the points of intersection of three sets of equidistant parallel planes dividing space into cubes. Fig. 2 shows O; and, at X, Y, Z, three of the six equidistant nearest neighbours which it has in the simple cubic arrangement. Keeping λ with the same signification in respect to fig. 2 as before, we have now for the coordinates of O's six nearest neighbours:

$$(\lambda\sqrt{2}, 0, 0), (0, \lambda\sqrt{2}, 0), (0, 0, \lambda\sqrt{2}),$$

$$(-\lambda\sqrt{2}, 0, 0), (0, -\lambda\sqrt{2}, 0), (0, 0, -\lambda\sqrt{2}).$$

Hence, and denoting by ϖ_1 the value of ϖ for this case, we find, by § 18 (23),

$$A = N\varpi_1 \quad \text{and} \quad B = n = 0 \text{ ....} \quad (31).$$

The explanation of $n = 0$ (facial rigidity zero) is obvious when we consider that a cube having for its edges twelve equal straight bars, with their ends jointed by threes at the eight corners, affords no resistance to change of the right angles of its faces to acute and obtuse angles.

§ 25. Replacing now the Boscovich points in the middles of the faces of the cubes, from which we supposed them temporarily annulled in § 24, and putting the results of § 23 and § 24 together, we find for our equilateral homogeneous assemblage its elasticity moduluses as follows:

$$\left.\begin{aligned} A &= N(\varpi_0+\varpi_1) \\ B &= n = \tfrac{1}{2}N\varpi_0 \end{aligned}\right\} \text{...............} \quad (32),$$

where, as we see by § 16 (20) above,

$$\left.\begin{aligned} \varpi_0 &= \lambda F(\lambda) - \lambda^2 F'\lambda \\ \varpi_1 &= \lambda\sqrt{2}F(\lambda\sqrt{2}) - 2\lambda^2 F'(\lambda\sqrt{2}) \end{aligned}\right\} \text{.........} \quad (33),$$

$F(r)$ being now taken to denote repulsion between any two of the points at any distance r, which, with $\phi(r)$ defined as in § 10, is the

* 'Math. and Phys. Papers,' vol. iii, p. 403.

meaning of $-\phi'(r)$. To render the solid, constituted of our homogeneous assemblage, elastically isotropic, we must, by § 19 (24), have $A-B=2n$, and therefore, by (32),

$$\varpi_0 = 2\varpi_1 \qquad (34).$$

§ 26. The last three of the six equilibrium equations § 16 (18) are fulfilled in virtue of symmetry in the case of an equilateral assemblage of single points whatever be the law of force between them, and whatever be the distance between any point and its nearest neighbours. The first three of them require in the case of § 23 that $F(\lambda)=0$; and in the case of (24) that $F(\lambda\sqrt{2})=0$, results of which the interpretation is obvious and important.

§ 27. The first three of the six equilibrium equations, § 16 (18), applied to the case of § 25, yield the following equation:—

$$\sqrt{\tfrac{1}{2}}F(\lambda\sqrt{2}) = -F(\lambda) \qquad (35);$$

that is to say, if there is repulsion or attraction between each point and its twelve nearest neighbours, there is attraction or repulsion of $\sqrt{2}$ of its amount between each point and its six next-nearest neighbours, unless there are also forces between more distant points. This result is easily verified by simple synthetical and geometrical considerations of the equilibrium between a point and its twelve nearest and six next-nearest neighbours in an equilateral homogeneous assemblage. The consideration of it is exceedingly interesting and important in respect to, and in illustration of, the engineering of jointed structures with redundant links or tie-struts.

§ 28. Leaving, now, the case of an equilateral homogeneous assemblage, let us consider what we may call a scalene assemblage, that is to say, an assemblage in which there are three sets of parallel rows of points, determinately fixed as follows, according to the system first taught by Bravais:—*

I. Just one set of rows of points at consecutively shortest distances λ_1.

II. Just one set of rows of points at consecutively next-shortest distances λ_2.

III. Just one set of rows of points at consecutive distances shorter than those of all other rows not in the plane of I and II.

To the condition $\lambda_3 > \lambda_2 > \lambda_1$ we may add the condition that none of the angles between the three sets of rows is a right angle, in order that our assemblage may be what we may call wholly scalene.

* 'Journal de l'École Polytechnique,' tome xix, cahier xxxiii, pp. 1—128: Paris, 1850.

§ 29. Let A′OA, B′OB, C′OC be the primary rows thus determinately found having any chosen point, O, in common; we have

$$\left.\begin{aligned} A'O = OA = \lambda_1 \\ B'O = OB = \lambda_2 \\ C'O = OC = \lambda_3 \end{aligned}\right\} \quad \ldots\ldots\ldots\ldots\ldots\ldots \quad (36).$$

Thus A′ and A are O's nearest neighbours; and B′ and B, O's next-nearest neighbours; and C′ and C, O's nearest neighbours not in the plane AOB. (It should be understood that there may be in the plane AOB points which, though at greater distances from O than B and B′, are nearer to O than are C and C′.)

§ 30. Supposing, now, BOC, B′OC′, &c., to be the acute angles between the three lines meeting in O; we have two equal and dichirally similar* tetrahedrons of each of which each of the four faces is a scalene acute-angled triangle. That every angle in and between the faces is acute we readily see, by remembering that OC and OC′ are shorter than the distances of O from any other of the points on the two sides of the plane AOB.†

§ 31. As a preliminary to the engineering of an incompressible elastic solid according to Boscovich, it is convenient now to consider a special case of scalene tetrahedron, in which perpendiculars from the four corners to the four opposite faces intersect in one point. I do not know if the species of tetrahedron which fulfils this condition has found a place in geometrical treatises, but I am informed by Dr. Forsyth that it has appeared in Cambridge examination papers. For my present purpose it occurred to me thus:—Let QO, QA, QB, QC be four lines of given lengths drawn from one point, Q. It is required to draw them in such relative directions that the volume of the tetrahedron OABC is a maximum. Whatever be the four given lengths, this problem clearly has one real solution and one only: and it is such that the four planes BOC, COA, AOB, ABC are cut perpendicularly by the lines AQ, BQ, CQ, OQ, respectively, each produced through Q. Thus we see that the special tetrahedron is defined by four lengths, and conclude that two equations among the six edges of the tetrahedron in general are required to make it our special tetrahedron.

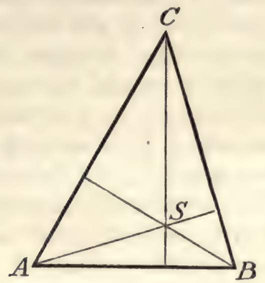
§ 32. Hence we see the following simple way of drawing a special tetrahedron. Choose as data three sides of one face and the length

* Either of these may be turned round so as to coincide with the image of the other in any plane mirror. Either may be called a pervert of the other; as, according to the usage of some writers, an object is called a *pervert* of another if one of them can be brought to coincide with the image of the other in a plane mirror (as, for example, a right hand and a left hand).

† See "Molecular Constitution of Matter," § (45), (*h*), (*i*), 'Math. and Phys. Papers,' vol. iii, pp. 412—413.

perpendicular to it from the opposite angle. The planes through this perpendicular, and the angles of the triangle, contain the perpendiculars from these angles to the opposite faces of the tetrahedron, and therefore cut the opposite sides of the triangle perpendicularly. (Thus, parenthetically, we have a proof of the known theorem of elementary geometry that the perpendiculars from the three angles of a triangle to the opposite sides intersect in one point.) Let ABC

FIG. 3.



be the chosen triangle and S the point in which it is cut by the perpendicular from O, the opposite corner of the tetrahedron. AS, BS, CS, produced through S, cut the opposite sides perpendicularly, and therefore we find the point S by drawing two of these perpendiculars and taking their point of intersection. The tetrahedron is then found by drawing through S a line SO of the given length perpendicular to the plane of ABC. (We have, again parenthetically, an interesting geometrical theorem. The perpendiculars from A, B, C to the planes of OBC, OCA, OAB cut OS in the same point; SO being of any arbitrarily chosen length.)

§ 33. I wish now to show how an incompressible homogeneous solid of wholly oblique crystalline configuration can be constructed without going beyond Boscovich for material. Consider, in any scalene assemblage, the plane of the line A′OA through any point O and its nearest neighbours, and the line B′OB through the same point and its next-nearest neighbours. To fix the ideas, and avoid circumlocutions, we shall suppose this plane to be horizontal. Consider the two parallel planes of points nearest to the plane above it and below it. The corner C of the acute-angled tetrahedron OABC, which we have been considering, is one of the points in one of the two nearest parallel planes, that above AOB we shall suppose. And the corner C′ of the equal and dichirally similar tetrahedron OA′B′C′ is one of the points in the nearest parallel plane below. All the points in the plane through C are corners of equal tetrahedrons chirally similar to OABC, and standing on the horizontal triangles oriented

as BOA. All the points C′ in the nearest plane below are corners of tetrahedrons chirally similar to OA′B′C′ placed downwards on the triangles oriented as B′OA′. The volume of the tetrahedron OABC is $\frac{1}{6}$ of the volume of the parallelepiped, of which OA, OB, OC are conterminous edges. Hence the sum of the volumes of all the upward tetrahedrons having their bases in one plane is $\frac{1}{6}$ of the volume of the space between large areas of these planes: and, therefore, the sum of all the chirally similar tetrahedrons, such as OABC, is $\frac{1}{6}$ of the whole volume of the assemblage through any larger space. Hence any homogeneous strain of the assemblage which does not alter the volume of the tetrahedrons does not alter the volume of the solid. Let tie-struts OQ, AQ, BQ, CQ be placed between any point Q within the tetrahedron and its four corners, and let these tie-struts be mechanically jointed together at Q, so that they may either push or pull at this point. This is merely a mechanical way of stating the Boscovichian idea of a second homogeneous assemblage, equal and similarly oriented to the first assemblage and placed with one of its points at Q, and the others in the other corresponding positions relatively to the primary assemblage. When it is done for all the tetrahedrons chirally similar to OABC, we find four tie-strut ends at every point O, or A, or B, or C, for example, of the primary assemblage. Let each set of these four ends be mechanically jointed together, so as to allow either push or pull. A model of the curious structure thus formed was shown at the conversazione of the Royal Society of June 7, 1893. It is for three dimensions of space what ordinary hexagonal netting is in a plane.

§ 34. Having thus constructed our model, alter its shape until we find its volume a maximum. This brings the tetrahedron, OABC, to be of the special kind defined in § 30. Suppose for the present the tie-struts to be absolutely resistant against push and pull, that is to say, to be each of constant length. This secures that the volume of the whole assemblage is unaltered by any infinitesimal change of shape possible to it; so that we have, in fact, the skeleton of an incompressible and inextensible solid.* Let now any forces whatever, subject to the law of uniformity in the assemblage, act between the points of our primary assemblage: and, if we please, also between all the points of our second assemblage; and between all the points of the two assemblages. Let these forces fulfil the conditions of equilibrium; of which the principle is described in § 16 and applied to find the equations of equilibrium for the simpler case of a single homogeneous assemblage there considered. Thus we have an incompressible elastic

* This result was given for an equilateral tetrahedronal assemblage in § 67 of "Molecular Constitution of Matter," 'Math. and Phys. Papers,' vol. iii, pp. 425–426.

solid; and, as in § 17 above, we see that there are fifteen independent coefficients in the quadratic function of the strain-components expressing the work required to produce an infinitesimal strain. Thus we realise the result described in § 7 above.

§ 35. Suppose now each of the four tie-struts to be not infinitely resistant against change of length, and to have a given modulus of longitudinal rigidity, which, for brevity, we shall call its stiffness. By assigning proper values to these four stiffnesses, and by supposing the tetrahedron to be freed from the two conditions making it our special tetrahedron, we have six quantities arbitrarily assignable, by which, adding these six to the former fifteen, we may give arbitrary values to each of the twenty-one coefficients in the quadratic function of the six strain-components with which we have to deal when change of bulk is allowed. Thus, in strictest Boscovichian doctrine, we provide for twenty-one independent coefficients in Green's energy-function. The dynamical details of the consideration of the equilibrium of two homogeneous assemblages with mutual attraction between them, and of the extension of §§ 9—17 to the larger problem now before us, are full of purely scientific and engineering interest, but must be reserved for what I hope is a future communication.

II. "Magnetic Qualities of Iron." By J. A. Ewing, M.A., F.R.S., Professor of Mechanism and Applied Mechanics in the University of Cambridge, and Miss Helen G. Klaassen, Lecturer in Physics, Newnham College. Received June 7, 1893.

(Abstract.)

The paper describes a series of observations of magnetic quality in various specimens of sheet iron and iron wire. A principal object was to determine the amount of energy lost in consequence of magnetic hysteresis when the iron under examination was carried through cyclic magnetising processes between assigned limits of the magnetic induction B. For this purpose observations of the relation of the induction B to the magnetic force H were made, from which curves were drawn, and the area enclosed by the curves in cyclic magnetising processes was measured. Many such cycles were gone through in the case of each of the specimens, the limits between which B was reversed being varied step by step in successive cycles, to allow the relation of the energy expended or of $\int Hd\mathrm{I}$ to B to be determined. The curves of B and H in these graded cycles are drawn in the paper, as well as curves showing the relation of $\int Hd\mathrm{I}$ to B and to H. Most of these experiments were made by the ballistic method, the specimens

being in the form of rings. The iron examined was, for the most part, thin sheet metal or wire such as is used in the construction of transformer cores. The experiments show that there are marked differences in the values of $\int HdI$ in different specimens, some nominally soft iron requiring two and even three times as much work to be spent in reversing its magnetism as is required in the best iron. They show, further, that great permeability does not necessarily imply small hysteresis losses. The order of merit in a group of samples is not the same when permeability is made the criterion of magnetic softness as it is when the smallness of $\int HdI$ is made the criterion.

In connexion with these results a formula proposed by Mr. C. P. Steinmetz ($\int HdI = cB^{1.6}$)* to express the relation of the hysteresis losses to B is discussed, and it is shown that although such a formula may serve fairly well as an approximate statement of the relation within those limits of B which are important in practice, it fails when applied to the more extreme portions of the curve.

The authors go on to describe a second group of experiments, in which direct measurements were made of the heat developed in magnetic reversals. The method consisted in using two rings, alike in all respects, with divided magnetising coils. One ring had its coils coupled so that the two parts opposed each other, and the core was consequently not magnetised when a current passed. The other ring was active, and its coils (coupled inductively) were connected in series with the non-inductive coils of the inactive ring. Alternating currents were passed through both, and the active ring became heated by the effects of hysteresis and Foucault currents. To balance this a steady current was caused to flow in the core of the inactive ring, and the energy expended in the magnetic reversals of the active ring was found by measuring the energy which had to be expended in this current in order that the temperature of the two rings might continue equal. In some cases the rings used were miniature transformers, and the test was applied to see whether the energy expended in magnetic reversals remained the same when the secondary of the transformer was closed as when it was open. This question had been raised by more than one experimentalist in relation to tests of the efficiency of transformers. The authors could not detect any difference in the amount of energy consumed in the core when the "load" was taken off or put on the secondary.

In a third group of experiments the magnetic curve tracer was used to examine certain features of the curves of magnetisation. This instrument, invented by one of the authors, draws curves which exhibit the relation of the magnetisation of given samples of iron or steel to

* 'Trans. American Inst. of Electrical Engineers,' vol. 9, No. 1.

the magnetising current. Amongst other points referred to in this connexion is the time-lag in magnetisation, which is well shown by the curve-tracer, and the effects are compared of the same cycle of magnetic force gone through at various speeds. It is shown that in solid bars 1·9 cm. in diameter, especially in soft iron, remarkable evidences of time-lag are seen, even when the period of magnetic reversal is as long as 3 secs. The work spent per cycle is a maximum at a particular frequency, which in such bars is very low.

The fourth and last section of the paper relates to the molecular theory of magnetisation, and describes experiments made with groups of small pivoted magnets. It is shown that the behaviour of such groups, when exposed to the action of a variable magnetic field, presents striking points of resemblance to the behaviour of iron or steel under corresponding variations of magnetising force. Results are given which tend to confirm the theory.

The particulars of the observations are set out in about forty sheets of curves which accompany the paper.

III. "Polarisation of Platinum Electrodes in Sulphuric Acid." By JAMES B. HENDERSON, B.Sc. Communicated by LORD KELVIN, P.R.S. Received June 10, 1893.

This investigation was begun about the beginning of February, 1893, at the instigation of Lord Kelvin, and was conducted in the Physical Laboratory of Glasgow University. The object of the investigation was to obtain the difference of potential between two platinum electrodes immersed in a solution of sulphuric acid immediately after the stoppage of a current which had been electrolysing the solution, and to find how this difference varied with a variation in the intensity of the current or in the strength of the solution.

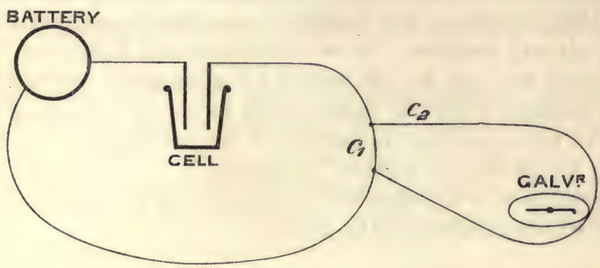
Former experiments by Buff ('Poggendorff,' vol. 130, p. 341, 1867) and Fromme ('Wiedemann,' vol. 33, p. 80, 1888) have given for the maximum polarisation with platinum wires of very small surface in the electrolysis of dilute sulphuric acid 3·5 and 4·6 volts.

Dr. Franz Richarz, in a paper "On the Polarisation of Small Electrodes in Dilute Sulphuric Acid," read before the British Association at Bath (1888), says of the above :—

"In these experiments the polarisation is calculated from measurements of the intensity of the galvanic current during the electrolysis, tacitly assuming that the resistance of the decomposition cell is independent of the intensity of the galvanic current. The correctness of the supposition has not been proved. I tried experiments by similar methods, and obtained yet greater values of the polarisation; it was calculated with a current density of 12 ampères per square

centimetre as 4·4 daniells (4·7 volts), and increased more and more with increasing intensity of the galvanic current. It is very improbable that this can be right. By supposing, however, that the resistance of the decomposition cell is not independent of the intensity, but decreases in a fixed manner with increasing intensity, the calculation of the same experiments gives small and constant values of polarisation."

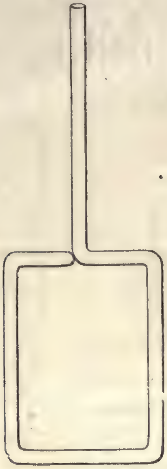
FIG. 1.



The method Dr. Richarz used to find the polarisation in his investigation was independent of the resistance of the electrolytic cell. The battery electrolytic cell and a switch, c_1, were joined in closed circuit. A branch circuit containing a very high resistance, a galvanometer, and another switch, c_2, joined the two sides of the switch c_1 (c_1 and c_2 were the two contacts of a Helmholtz's pendulum interrupter). When c_1 was made there was a very small current through the galvanometer. To determine the polarisation c_1 was broken, and immediately after c_2 also. In the short time between the interruption of c_1 and c_2 a current strong for the sensibility of the galvanometer went through it. The polarisation was calculated from the deflection given to the galvanometer needle by the impact of the current, which was proportional to the electromotive force of the battery *minus* the polarisation. In this way Dr. Richarz found values for the polarisation never greater than 2·6 volts with small wire electrodes, and also got the same maximum with large platinum plates.

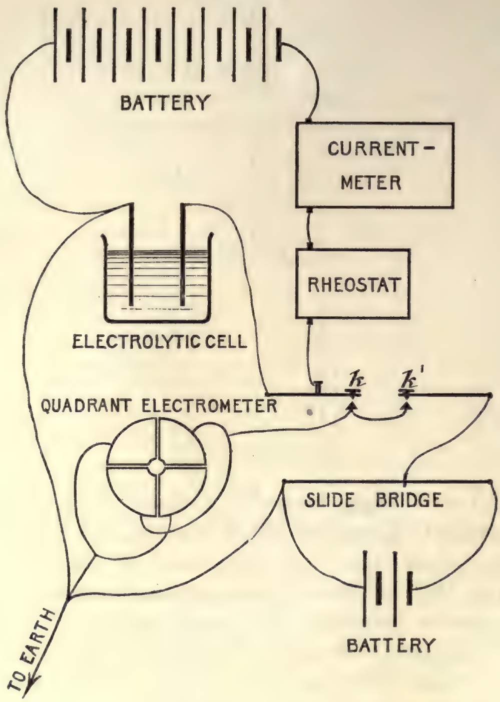
The cell used in the present investigation was a cylindrical glass vessel 10 cm. diameter and 12 cm. deep. The electrodes were rectangular plates of platinum foil 7 cm. long by 5·5 cm. broad, and were stiffened by being mounted on rectangular frames made by bending glass tubing (fig. 2). The tubing of these frames also served to support the plates in vertical planes by being passed through holes in a bar of wood placed across the mouth of the vessel. The plates were immersed in the solution to a depth of 5 cm., having their planes parallel and about 1 cm. apart. There were thus 55 sq. cm. of surface of each plate wetted. To find the polari-

FIG. 2.



sation one of Lord Kelvin's quadrant electrometers was used, and by an arrangement, described later, the breaking of the electrolysing current circuit and the switching of the electrodes on to the terminals of the electrometer were done simultaneously. Before switching as above, however, the needle of the electrometer was deflected by making a difference of potential between the pairs of quadrants, and this deflection was so adjusted by trial and error that, when the electrodes were switched on, the needle was no farther deflected. For deflecting the needle of the electrometer a high resistance slide bridge was used. A difference of potential was maintained between its two ends, and the difference of potential between one end and the slider was used to deflect the needle, so that by moving the slider one way or the other the deflection could be increased or diminished at will. The electrolysing current was kept constant throughout each experiment, being measured by one of Lord Kelvin's electric balances and adjusted by a rheostat. One terminal of the electrometer, one electrode, and one end of the slide bridge were connected together and then put to earth. The current for the electrolysis was got from eight large secondary cells, and the difference of potential between the ends of the slide bridge was maintained by two small secondary cells. The arrangement of keys can be best understood from the diagram. By pressing the key k', connexion was made between the slider and the unearthed quadrants, and when the key k was free, the circuit was complete for the electrolysing current, but when k was pressed down the circuit was broken, and the unearthed electrode was connected to the unearthed quadrants.

Fig. 3.



The order of an experiment was the following.

After carefully standardising the electrometer, the electrolysis was started, and the unearthed electrode connected by a wire (not shown in the diagram) to the unearthed quadrants. The deflection of the needle thus produced, which showed the difference of potential between the electrodes, continued to increase steadily until, after the lapse of an interval of time depending on the strength of the current, it became constant. When this stage was reached, the wire mentioned above was removed and the key k' pressed and kept down, thus making connexion between the slider and the quadrants. The slider was then moved along until the deflection was nearly equal to that which would be given by the polarisation, and the key k momentarily pressed, thereby breaking the current circuit and connecting the electrode to the quadrants. An impulsive deflection immediately followed, unless the potential of the quadrants was equal to that of polarisation. If this deflection was negative (which indicated that the potential of polarisation was less than that of the quadrants) the slider was moved so as to reduce the potential of the quadrants below that of polarisation, thereby making the impulsive

deflection positive, and then the experiment was continued as below. When the positive deflection was obtained its amount was noted, and the slider was moved so as to increase the steady deflection nearly up to the point on the scale reached by the impulsive one, and another trial then made. In this way, by watching the point reached by each impulsive deflection, and then increasing the steady one almost up to that point, the latter was increased until the former vanished, that is, until the potential of the quadrants was that of polarisation. The magnitude of this deflection was then noted, and the polarisation calculated from it. In these trials the key k was kept down only for about two seconds, just sufficient time to allow the extent of the deflection to be seen, and at least two minutes were allowed to elapse between one trial and the next.

After the maximum deflection had been reached, a considerable interval of time was allowed to elapse, and then the key k' raised and k simultaneously lowered and kept down, and the rate of fall of the deflection noted. The above motion of the keys threw the slider off and put the electrode on to the quadrants, at the same time stopping the current. The deflection was therefore due to polarisation alone, and its rate of fall was therefore the rate of fall of the polarisation.

The results of one series of experiments are given in the accompanying table.

All the results point to the polarisation being constant with large electrodes, being independent of the strength of the solution and the intensity of the current. The variations in the figures do not occur

Percentage strength of solution.	Strength of current in ampères.	Time the current had been passing.	Polarisation in volts.
		h. m.	
30	0·2	3 25	2·066
,,	0·5	0 45	2·060
,,	1·0	0 35	2·060
,,	1·0	0 45	2·124
20	0·1	3 22	2·126
,,	0·5	1 25	2·139
,,	1·0	0 25	2·090
,,	1·0	0 35	2·124
10	0·1	17 40	2·139
,,	0·5	1 19	2·066
,,	1·0	0 44	2·066
5	0·1	18 30	2·116
,,	0·5	1 36	2·078
,,	1·0	1 0	2·083
,,	1·0	3 15	2·054

Mean polarisation = 2·09 volts.

in any order, and are all such as might be expected in experimental results of this nature. Some of the greatest variations were obtained in exactly similar experiments performed at different times.

The mean of all the values of the polarisation in this table is 2·09 volts.

The rate of fall of the polarisation depends on the time the current has been electrolysing the solution, and also on its intensity, but in every case the fall is very rapid at first, being in some cases as much in the first minute as it is in the next five minutes, and the fall in the first minute is never less than one-fourth of the polarisation.

IV. "On the Annual and Semi-annual Seismic Periods." By CHARLES DAVISON, M.A., Mathematical Master at King Edward's High School, Birmingham. Communicated by Professor J. H. POYNTING, F.R.S. Received June 13, 1893.

(Abstract.)

Method of Investigation.—The method adopted is similar to that employed by Dr. C. G. Knott in his paper on "Earthquake Frequency."

If $f(\theta)$ be a periodic function of θ, then

$$f(\theta) = a_0 + a_1\cos(\theta+\alpha_1) + a_2\cos(2\theta+\alpha_2) + \ldots . + a_n\cos(n\theta+\alpha_n) + \ldots ,$$

from which it follows that

$$\frac{1}{\pi}\int_{\theta-\pi/2}^{\theta+\pi/2} f(\theta)\,d\theta = a_0 + \frac{2a_1}{\pi}\cos(\theta+\alpha_1) - \frac{2a_3}{3\pi}\cos(3\theta+\alpha_3) + \ldots . + \frac{2a_n\sin\frac{n\pi}{2}}{n\pi}\cos(n\theta+\alpha_n) + \ldots .$$

This latter expression gives the mean value of $f(\theta)$ through an interval $\pi/2$ on either side of θ. From it, all terms involving even multiples of θ are eliminated, and the coefficients of all terms after the second are diminished to a greater extent than that of the second.

A definition of the unit earthquake having been adopted, the earthquakes of different districts are classified in half-monthly groups, the first half of February containing fourteen days, and of all the other months fifteen days; and the numbers so obtained are reduced to intervals of equal length (fifteen days). The numbers for the two halves of each month are added together. The mean of the numbers for the six months from November to April gives the six-monthly

mean corresponding to the end of January. Six-monthly means are calculated in this way for the end of each month; each mean is divided by the average of all twelve, and the difference between each quotient and unity is multiplied by the augmenting factor 1·589, in order to obtain the correct value of the ratio $a_1 : a_0$. The curve obtained by plotting these reduced means thus gives special prominence to the annual period, by eliminating the semi-annual period and all those which are fractions of six months, and by diminishing the amplitudes of all other periods with respect to that of the annual period.

In investigating the semi-annual period, the numbers corresponding to the first halves of January and July are added together, and so on; the rest of the method being the same as for the annual period. The result gives special prominence to the semi-annual period by eliminating the annual period, and by eliminating or diminishing the amplitudes of all periods less than six months.

Seismic Periodicity in relation to Intensity.—This discussion is founded on: (1) lists compiled from Mallet's great catalogue, first, of shocks which were so slight as to be just perceptible, and, secondly, of those which were strong enough to damage buildings; (2) Professor Milne's classification of the Japanese earthquakes of 1885 to 1889 according to the areas disturbed by them; and (3) different catalogues relating to the same district, it being obvious that two such catalogues for the same time can only differ by the omission or inclusion of slight shocks.

The following results are obtained:—(1) In both periods, the amplitude is greater for slight than for strong shocks; (2) there appear to be two classes of slight shocks with an annual period, the stronger having their maximum in winter, the weaker in summer; and (3) in the case of the semi-annual period, both strong and slight shocks, as a rule, have nearly the same maximum epochs.

Seismic Periodicity in relation to Geographical Position.—The number of records examined is 62, 45 belonging to the northern hemisphere, 14 to the southern, and 3 to equatorial countries.

1. *Annual Period.*—In every district, and in all but five records (which are obviously incomplete), there is a fairly well-marked annual period. As a rule, different records for the same district agree in giving the same, or nearly the same, maximum epoch. Excluding, however, those which disagree in this respect, we have left 34 records for the northern hemisphere, 9 for the southern, and 2 for equatorial countries. In the northern hemisphere, 4 records give the maximum in November, 16 in December, and 6 in January; in the southern hemisphere, 2 in April, 2 in May, 3 in July, and 2 in August; the end of the month being supposed in each case. As a rule, then, the maximum epoch occurs in winter in both hemispheres. The ampli-

tude of the annual period ranges from 0·05 (New Zealand) to 0·67 (Sicily and Algeria), the average of 57 records being 0·33.

2. *Semi-annual Period.*—Of the 62 records examined, only 3 fail to show a semi-annual period, the cause of the failure in these cases being no doubt the imperfection of the seismic record. In New Zealand and South-east Australia, the maximum epoch generally falls either in February or March and August or September; in North America, as a rule, in March or April and September or October. But for other regions it does not seem possible as yet to deduce any law. The amplitude of the semi-annual period ranges from 0·06 (southern hemisphere) to 0·79 (Mexico), the average value being 0·24.

3. In fifteen cases, the amplitude of the semi-annual period exceeds that of the annual period. Eleven of these records include the following insular districts, which are among the most well-marked seismic regions in the world, namely, the Grecian Archipelago, Japan, the Malay Archipelago, New Zealand, and the West Indies. The average amplitude of the annual period in these eleven cases is 0·16, and that of the semi-annual period 0·24; *i.e.*, the average amplitude of the annual period is just half that for all the districts examined, while in the case of the semi-annual period the average amplitudes are the same.

Origin of the Annual Period.—In this, the concluding, section of the paper, an attempt is made to show that the annual change in barometric pressure may be the cause of the annual change in seismic frequency. It would be difficult to prove that such a connexion exists, but reasons are given which seem to render it in some degree probable.

1. The most probable cause of the origin of the majority of non-volcanic earthquakes is the impulsive friction, due to slipping, of the two rock-surfaces of a fault. Now, whatever be the causes of seismic periodicity, it seems probable that they are merely auxiliary, and determine the epoch when an earthquake shall take place, rather than there shall be an earthquake at all. Professor G. H. Darwin has shown that the vertical displacement of the earth's surface by parallel waves of barometric elevation and depression is not inconsiderable, and that it diminishes at first very slowly as the depth increases. Since the fault-slip which produces even a moderately strong shock must be very small, and since the work to be done in such a case is, not the compression of solid rock, but the slight depression of a fractured mass whose support is nearly, but not quite, withdrawn, the annual range of barometric pressure does not seem incompetent to produce the effects observed.

2. Comparisons between the dates of the maximum epochs of the seismic and barometric annual periods are made in 31 of the districts treated in this paper. The seismic maximum approximately coincides

with the barometric maximum in 10 districts, and follows it by about one month in 9, and by about two months in 4, districts; the other cases generally admitting of some explanation.

3. In several insular seismic districts, and especially in Japan and New Zealand, the amplitude of the annual period is very small; and, if many of the earthquakes of these districts originate beneath the sea, this should be the case; for, in the course of a year, as the barometric pressure changes, the sea will have time to take up its equilibrium position, and thus the total pressure on the sea-bottom will be unaltered.

V. "Electrical Interference Phenomena somewhat Analogous to Newton's Rings, but exhibited by Waves passing along Wires of which a part differs from the rest." By EDWIN H. BARTON, B.Sc., "1851 Exhibition" Science Scholar. Communicated by Professor A. W. RÜCKER, M.A., F.R.S. Received May 25, 1893.

1. In 1891 Mr. V. Bjerknes* showed how to measure the wave-length and primary damping of the electrical oscillations in a Hertzian primary conductor by the use of a special electrometer and long parallel wires along which induced oscillations were propagated. This form of Hertzian secondary conductor, in which the wires are far too long to be in resonance with the primary oscillator, may, hereafter in this paper, be referred to as "the long secondary" or simply "the secondary."

2. During the following session Herr von Geitler† found that, if the wires at any part of the long secondary were either

(1) Replaced by others thicker or thinner than the normal wires, or

(2) Arranged nearer together or further apart than the normal distances,

then in any of these cases, a partial reflection of the electrical waves occurred at such place of change in the wires.

Herr von Geitler then made further observations of what occurred when a condenser was attached at a *single point* of each wire, but did not quantitatively examine the effect produced on the waves by a finite length of the secondary being different from the rest.

* 'Wiedemann's Annalen,' vol. 44, pp. 513—526, 1891, "Ueber den Zeitlichen Verlauf der Schwingungen im primären Hertz'schen Leiter," von V. Bjerknes aus Christiania.

† 'Wiedemann's Annalen,' vol. 49, pp. 184—195, 1893, "Ueber Reflexion electrischer Drahtwellen," von J. Ritter von Geitler.

3. Following the researches of these physicists, and in the same laboratory,* I have endeavoured to trace, both theoretically and experimentally, the relation between the length of this abnormal part of the "secondary" and the relative intensities of the transmitted and reflected disturbances into which the original incident wave is thereby divided.

4. The following diagram and accompanying descriptive notes will sufficiently explain the apparatus used in the experiments :—

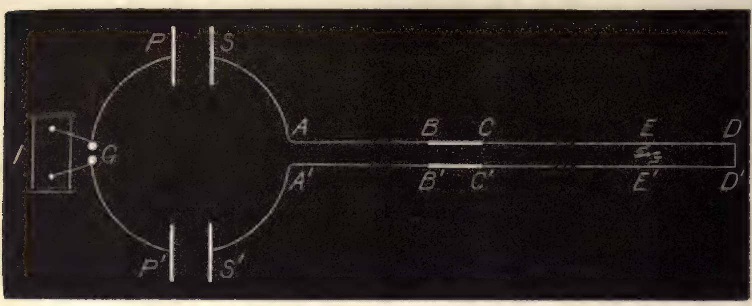


Fig. 1.—Diagrammatic Outline of the Apparatus used for producing and measuring the Interference of Electrical Oscillations.

EXPLANATION OF FIG. 1.

I. Induction coil worked by two secondary cells.

G. Spark gap (usually 2 mm.).

PGP′. (Measured along the wires) is 204 cm., the wires PG, GP′ are 2 mm. diameter.

PP′. Condenser plates of zinc 40 cm. diam. to form the ends of the Hertzian primary oscillator.

SS′. Similar plates at a distance of 30 cm. from P and P′, and forming the beginning of the "long secondary," which consists of copper wires 1 mm. diameter.

Distance AA′ = BB′ = CC′ = DD′ = 8 cm.

BC B′C′. The abnormal part of the long secondary used to produce the reflection and interference phenomena.

EE′. Electrometer.† The needle is uncharged: it therefore turns in the same direction whenever there is any potential difference between E and E′, whatever the sign of that difference.

DD′. Wire bridge across the main wires.

Length AD = A′D′ = 160 m. nearly. ED = $\frac{1}{4}\lambda_1$ where λ_1 denotes the wave-length in the long secondary.

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* Namely, in the University of Bonn, under the guidance of Professor Hertz.

† Herr von Geitler kindly left for my use the instrument made by him and described in 'Wiedemann's Annalen,' vol. 49, p. 188. I am also indebted to him for full verbal explanations of his researches prior to their publication, and hereby tender him my hearty thanks.

Divisions of Theory.

5. Imagine an electrical conductor ABCD (see fig. 1) consisting of three parts, AB, BC, and CD, in each of which parts the electrostatic capacity and other properties of the conductor remain constant, those of the third part CD being precisely like those of the first part AB, but the second part BC differing from the other two parts either in its own dimensions or in the nature of the dielectric* by which it is surrounded, or in both these respects.

And consider an electrical wave passing along this conductor from A towards D. Suppose its amplitude in the part AB is a, and that from the point B (immediately after the instant of incidence of the wave there and *before any disturbance has reached* C), a wave of amplitude ab is reflected towards A, and a wave of amplitude ac transmitted within the part BC towards C. The constants b and c may be referred to as the *coefficients of reflection* and *transmission* respectively.

6. Then the elementary mathematical investigation of what may be expected to occur falls naturally into three parts, namely :—

(1) The derivation of the coefficients of reflection and transmission at the point B from the changes in the properties of the conductor which occur there.

(2) The relation between these coefficients and the similar ones involved when the wave reaches C and thus encounters the reverse change in the properties of the conductor.

(3) The determination of the intensities of the *total* disturbances reflected at or passing through B in the direction A and of those transmitted through C in the direction D respectively, each being the result of an infinite series of interfering waves produced by multiple reflections within the part BC, these again being produced by the original wave passing along AB.

These three branches of the theory will now be taken in the above order.

7. I. *Theory of single reflection and transmission of an electrical wave along a conductor at a point where either its electrostatic capacity, or its coefficient of self-induction, or both, change abruptly.*—Let the following symbols be used, the same system of units being understood throughout :—

ϕ. Electrostatic potential.

C. Electrostatic capacity per *unit length* of conductor.

* It is to be distinctly understood throughout that the medium surrounding *all parts* of the conductor is supposed to be a *dielectric.* All idea of its possessing *appreciable conductivity*, and consequently *absorbing a sensible portion of the energy* of the wave, is excluded from this theory. Also for all parts of the conductor itself let w (the magnetic permeability) = unity.

Q. Quantity of electricity per *unit length* of conductor.
L. Coefficient of self-induction " "
R. Resistance " "
i. Electric current.
v. Velocity of propagation of the waves along the conductor.
λ. Wave-length.
t. Time.
L, R, and v denote the values corresponding to the high frequencies used.
Take the conductor as the axis of x.

For the normal parts of the conductor, namely, AB and CD, fig. 1, the above symbols will be used with the subscript 1; for the abnormal part BC they will be used with the subscript 2.

8. When an electrical wave passes along a conductor we have at any point the E.M.F. $= -\frac{\partial\phi}{\partial x} - L\frac{\partial i}{\partial t}$. But this also equals R$i$. Thus, since $i = Qv = C\phi v$, we obtain the differential equation

$$\frac{\partial\phi}{\partial x} + vCL\frac{\partial\phi}{\partial t} + vCR\phi = 0 \quad \ldots\ldots\ldots\ldots \quad (1).$$

When and where $\phi = 0$, we have

$$\frac{\partial\phi}{\partial x} + vCL\frac{\partial\phi}{\partial t} = 0 \quad \text{or} \quad \frac{\partial\phi}{\partial t}\left[vCL - \frac{1}{v}\right] = 0,$$

whence

$$v = \frac{1}{\sqrt{(CL)}} \quad \ldots\ldots\ldots\ldots\ldots\ldots\ldots \quad (2),$$

the well known expression for the velocity of propagation of the wave.

9. Now R in equation (1) leads to a damping factor in the solution. Since, however, we are now concerned simply with what occurs at the point of reflection, this R will be omitted. Equation (1) then becomes

$$\left.\begin{array}{l} v\dfrac{\partial\phi}{\partial x} + \dfrac{\partial\phi}{\partial t} = 0 \\ \text{and its solution} \\ \phi = f_1(\beta t - \beta_0 x) + f_2(\beta t + \beta_0 x) \end{array}\right\} \quad \ldots\ldots\ldots \quad (3),$$

where $\beta/\beta_0 = v = 1/\sqrt{(CL)}$, and f_1, f_2 denote any functions.

10. It will suffice for the case in question if we write for f_1 and f_2 sine functions with coefficients for the various amplitudes, and a third term in the brackets to allow for a change of phase should there be one. We may thus write for the original and reflected waves in the first part of the conductor

$$\phi_1 = a\sin(\beta t - \beta_1 x) + ab\sin(\beta t + \beta_1 x + \delta_1) \quad \ldots\ldots \quad (4).$$

And for the transmitted wave in the second part of the conductor

$$\phi_2 = ac \sin(\beta t - \beta_2 x + \delta_2) \ldots\ldots\ldots\ldots \quad (5),$$

where $\beta/\beta_1 = v_1$ and $\beta/\beta_2 = v_2$.

Further, remembering that $i = Q \times (\pm v) = C\phi \times (\pm v)$, we have from (4) and (5)

$$i_1 = C_1 v_1 a \sin(\beta t - \beta_1 x) - C_1 v_1 ab \sin(\beta t + \beta_1 x + \delta_1) \ldots \quad (6),$$

and $$i_2 = C_2 v_2 ac \sin(\beta t - \beta_2 x + \delta_2) \ldots\ldots\ldots\ldots \quad (7).$$

11. Now take B, the junction of the two parts of the conductor, as the origin of abscissæ. Then, for $x = 0$, we have $\phi_1 = \phi_2$ and $i_1 = i_2$; unless $\partial\phi/\partial x$ and $\partial i/\partial x$ become infinite at that point. Applying this relation to equations (4), (5), (6), and (7) yields

$$\sin \beta t + b \sin(\beta t + \delta_1) = c \sin(\beta t + \delta_2) \ldots\ldots \quad (8).$$

$$C_1 v_1 [\sin \beta t - b \sin(\beta t + \delta_1)] = C_2 v_2 c \sin(\beta t + \delta_2) \ldots \quad (9).$$

12. Now equations (8) and (9) hold good for all values of t; hence in each of them we may equate the coefficients of $\sin \beta t$ and of $\cos \beta t$ respectively after expanding the sines. This leads to four equations from which to determine the four unknowns. The solution may be written as follows:—

$$\left.\begin{aligned} &\delta_1 = 0, \qquad \delta_2 = 0, \\ &b = \frac{C_1 v_1 - C_2 v_2}{C_1 v_1 + C_2 v_2} = \frac{\sqrt{(C_1/L_1)} - \sqrt{(C_2/L_2)}}{\sqrt{(C_1/L_1)} + \sqrt{(C_2/L_2)}} \\ &c = \frac{2\, C_1 v_1}{C_1 v_1 + C_2 v_2} = \frac{2\sqrt{(C_1/L_1)}}{\sqrt{(C_1/L_1)} + \sqrt{(C_2/L_2)}} \end{aligned}\right\} \ldots \quad (10).$$

13. The following results of (10) may be noted:—

(1) Energy of the original wave is proportional to a constant $\times C_1 v_1 a^2$, that of the reflected wave to a constant $\times C_1 v_1 a^2 b^2$, and that of the transmitted wave to a constant $\times C_2 v_2 a^2 c^2$. We ought, therefore, to have—

$$C_1 v_1 = C_1 v_1 b^2 + C_2 v_2 c^2 \ldots\ldots\ldots\ldots \quad (11).$$

And this equation is satisfied by the values of b and c in (10).

(2) If $L_2 = L_1$ we have—

$$\left.\begin{aligned} &b = \frac{\sqrt{C_1} - \sqrt{C_2}}{\sqrt{C_1} + \sqrt{C_2}} = -\frac{v_1/v_2 - 1}{v_1/v_2 + 1} \\ \text{and} \quad &c = \frac{2}{\sqrt{C_1} + \sqrt{C_2}} = \frac{2}{v_1/v_2 + 1} \end{aligned}\right\} \ldots\ldots \quad (12).$$

[Compare Preston's 'Theory of Light,' 1890, pp. 285—286.]

(3) If $C_2 L_2 = C_1 L_1$, then $v_2 = v_1$, $\lambda_2 = \lambda_1$, and we have

$$\left.\begin{aligned} b &= \frac{C_1 - C_2}{C_1 + C_2} = -\frac{C_2/C_1 - 1}{C_2/C_1 + 1} \\ \text{and} \qquad c &= \frac{2\,C_1}{C_1 + C_2} = \frac{2}{C_2/C_1 + 1} \end{aligned}\right\} \ldots\ldots\ldots\ldots \quad (13).$$

14. II. *Relation between the various coefficients involved when the second part of the conductor has finite length, and is succeeded by a third part like the first.*—This abnormal intermediate part of the conductor may sometimes be referred to as "the condenser."

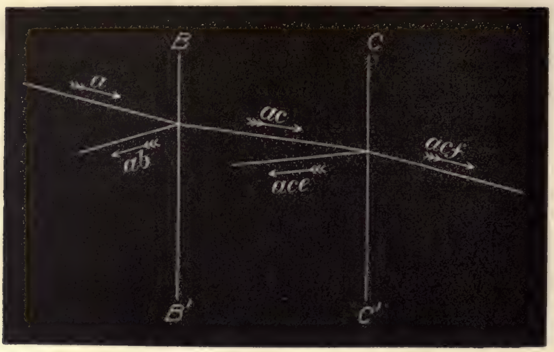


Fig. 2.—Diagrammatic View of Waves, drawn, for clearness' sake, as though they were rays incident and reflected obliquely. BB′ shows the boundary between the first and second parts of the conductor; CC′ shows that between the second and third parts. The small letters near the arrows are the amplitudes of the waves.

The values of b and c in fig. 2 have already been determined, and we have by the same equation [viz.: (10)]

$$\left.\begin{aligned} e &= \frac{C_2 v_2 - C_1 v_1}{C_2 v_2 + C_1 v_1} = -b, \\ \text{and} \qquad f &= \frac{2\,C_2 v_2}{C_2 v_2 + C_1 v_1}, \\ \text{whence} \qquad cf &= \frac{4\,C_1 v_1 C_2 v_2}{(C_1 v_1 + C_2 v_2)^2} = 1 - b^2. \end{aligned}\right\} \ldots\ldots\ldots\ldots \quad (14).$$

15. III. *Theory of the multiple internal reflections of a damped electrical wave in the abnormal part of a conductor (the previous and succeeding parts being alike) with expressions for the intensities of the resulting transmitted and reflected disturbances.*—Let the equation of the wave in the first part of the conductor be

$$y = ae^{-\alpha t + \alpha_1 x} \sin(\beta t - \beta_1 x) \ldots\ldots\ldots\ldots \quad (15),$$

where $\qquad \alpha/\alpha_1 = \beta/\beta_1 = v_1.$

(Similarly $\alpha/\alpha_2 = \beta/\beta_2$ will be used for the value v_2.)

It will thus be seen that the damping just referred to in the heading of this paragraph is the *primary damping*, that is, the time-rate of decrease of the oscillations occurring in the Hertzian *primary* conductor. *Secondary damping*, on the other hand, refers to the *space-rate* of decrease of any individual wave as it proceeds along the Hertzian *secondary* conductor. It is, of course, along the long form of this secondary conductor that we are now supposing the waves to travel, but the secondary damping is known to be small in comparison with the primary damping, and is, therefore, in the present part of the theory, legitimately neglected.*

16. It will readily be seen that the ordinary mathematical treatment of the interference of light in thin plates will not strictly apply to this case.

For, in the optical phenomenon, one supposes a continuous beam of light of constant amplitude. We may, therefore, in that case, at once take, to *infinity*, the sums of the series of reflected and transmitted rays to which the original one gives rise, and *neglect* the comparatively *small period which elapses before those two infinite series are made up*, and during which (the series being as yet incomplete) the reflected and transmitted beams have not reached their final steady values.

But with such primary damping as that with which we have to do (namely, of the order $\gamma_1 = 2\pi\alpha/\beta = 0{\cdot}5$), the character of the result would be essentially changed by the unwarrantable assumption that the amplitude of the incident wave remains sensibly constant until the infinite series of internal reflections has taken place.

17. The question is therefore attacked by forming a series of integrals.

Referring to fig. 1 or fig. 2, the wave given by equation (15) advances in the positive direction along the axis of x, that is, in the direction ABCD. Let $t = 0$ when the head of the wave first reaches C. And let $x = 0$ for the wave which is at the point C, *without having suffered any internal reflections in the part* BC. Thus, for a wave which has suffered $2n$ internal reflections within the part BC, the point C has for its abscissa $2n \times l$, where l denotes the length BC.

Now let y_n denote a wave emerging at C after $2n$ internal reflections in BC; then we have, by putting, in equation (15), $x = 2nl$, and supplying the amplitude from (14),

$$\left.\begin{array}{ll} & y_n = ab^{2n}(1-b^2)\, e^{-\alpha t+\alpha_2 2nl} \sin(\beta t - \beta_2 2nl) \\ \text{or} & y_n = ab^{2n}(1-b^2)\, e^{-\alpha(t-nt_2)} \sin[\beta(t-nt_2)], \\ \text{where} & t_2 = 2l/v_2 \end{array}\right\} \ldots (16).$$

* Compare 'Wiedemann's Annalen,' vol. 44, pp. 83 and 515, 1891.

It must now be recollected that, since the electrometer needle is uncharged, it takes no account of the sign of the potential difference between E and E′, fig. 1, but gives a deflection proportional to $\int y^2 dt$, taken between the proper time limits.

18. Hence, if E denotes the electrometer constant, and I_0, I_t, and I_r, its deflections for the passage of the wave-train, without the condenser, after transmission through the condenser (as shown in fig. 1), and by reflection from the condenser, respectively, we have

$$\left.\begin{aligned} EI_0 &= \int_0^\infty y_0^2\,dt \\ \text{and} \qquad EI_t &= \int_0^{t_2} y_0^2\,dt + \int_{t_2}^{2t_2} (y_0+y_1)^2\,dt + \int_{2t_2}^{3t_2} (y_0+y_1+y_2)^2\,dt \\ &\quad + \ldots . + \int_{nt_2}^{(n+1)t_2} (y_0+y_1+y_2+\ldots . +y_n)^2\,dt \\ &\quad + \ldots . \textit{ ad inf.} \end{aligned}\right\} \quad (17).$$

And in these equations everything is expressible in terms of quantities considered known.

19. The evaluation of the second part of (17) (being a doubly-infinite series of definite integrals) is a somewhat long process, but rigidly performed to infinity, and the result divided by that for EI_0 to eliminate E, and the like operation for I_r, we have

$$\left.\begin{aligned} I_t/I_0 &= \frac{1-b^2}{1+b^2} + U, \\ I_r/I_0 &= \frac{2b^2}{1+b^2} - U, \\ \text{where} \qquad U &= \frac{1-b^2}{1+b^2} \cdot \frac{2b^2 e^{-\alpha t_2}}{\beta} \cdot \frac{(\alpha \sin \beta t_2 + \beta \cos \beta t_2 - \beta b^2 e^{-\alpha t_2})}{1 - 2b^2 e^{-\alpha t_2} \cos \beta t_2 + b^4 e^{-2\alpha t_2}} \end{aligned}\right\} \quad (18).$$

20. The following results of equation (18) may be noticed.

(1) $I_t/I_0 + I_r/I_0 = 1$ or $I_t + I_r = I_0$ for *all* values of b and t_2, as should be the case.

(2) On putting $t_2 = 0$ or $b = 0$, that is, removing the condenser, we have

$$I_t/I_0 = 1 \text{ or } I_t = I_0, \text{ all transmitted,}$$

and

$$I_r = 0, \text{ none reflected.}$$

On the other hand, with $b = 1$, we get all reflected and none transmitted, unless $t_2 = 0$.

(3) On differentiating U to t_2, and putting $\partial U/\partial t_2 = 0$ to obtain the values of l which give the stationary values of I_t and I_r, we obtain $\sin \beta t_2 = 0$, whence $l = \frac{1}{4}(n\lambda_2)$, where n is any integer.

The values occur as in the following table :—

Values of l.	0.	$\frac{\lambda_2}{4}$.	$\frac{\lambda_2}{2}$.	$3\frac{\lambda_2}{4}$.	λ_2.	$2n\frac{\lambda_2}{4}$.	$(2n+1)\frac{\lambda_2}{4}$.
Nature of value of I_t	max. = I_0.	min.	max.	min.	max.	max.	min.
Nature of value of I_r.	min. = 0.	max.	min.	max.	min.	min.	max.

(4) We see, on inspection of (18), that the values of I_t and I_r (when plotted as the ordinates, the values of l being the abscissæ) form a damped wavy curve, but that neither the damping nor the wave-form are of the simplest type.

(5) On putting $t_2 = \infty$, U disappears, and we have

$$I_t/I_0 = \frac{1-b^2}{1+b^2}, \qquad I_r/I_0 = \frac{2b^2}{1+b^2} \quad \ldots\ldots\ldots \quad (19).$$

(6)* On putting $\alpha = 0$, that is, removing the primary damping from the expression for the wave, we obtain

$$\left.\begin{aligned} I_t/I_0 &= \frac{(1-b^2)^2}{1-2b^2\cos\beta t_2+b^4} \\ \text{and} \qquad I_r/I_0 &= \frac{4b^2\sin^2(\beta t_2/2)}{1-2b^2\cos\beta t_2+b^4} \end{aligned}\right\} \quad \ldots\ldots\ldots\ldots \quad (20),$$

the ordinary expressions for the case of the interference of light in thin plates (see, *e.g.*, Preston's 'Theory of Light,' 1890, pp. 145—147).

21. The theoretical values of I_t/I_0 from equation (18) are plotted in curve No. 1, for the following values of the constants, the abscissæ representing l, and the ordinates I_t/I_0.

$$\text{log. dec. } \gamma_1 = 2\pi\alpha/\beta = 0{\cdot}5,$$

$$C_2L_2 = C_1L_1;$$

therefore

$$\lambda_2 = \lambda_1 \text{ and } v_2 = v_1,$$

$$\lambda_1 = 9 \text{ m.},$$

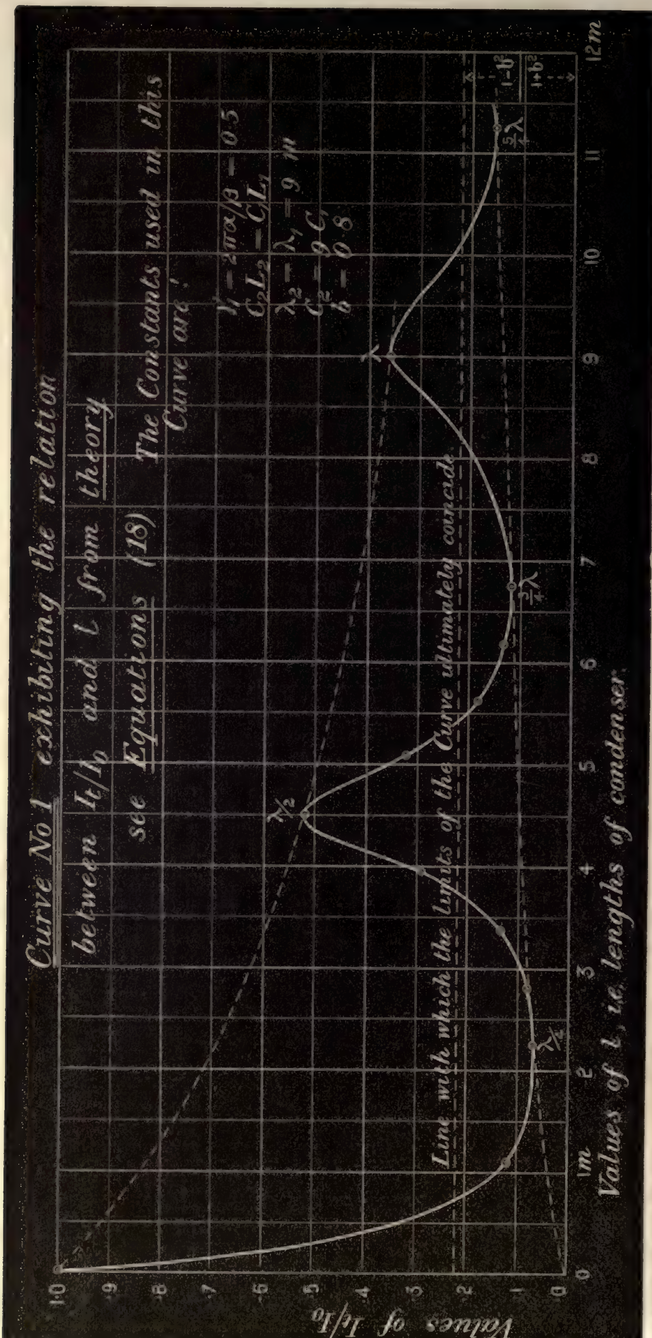
$$C_2 = 9C_1,$$

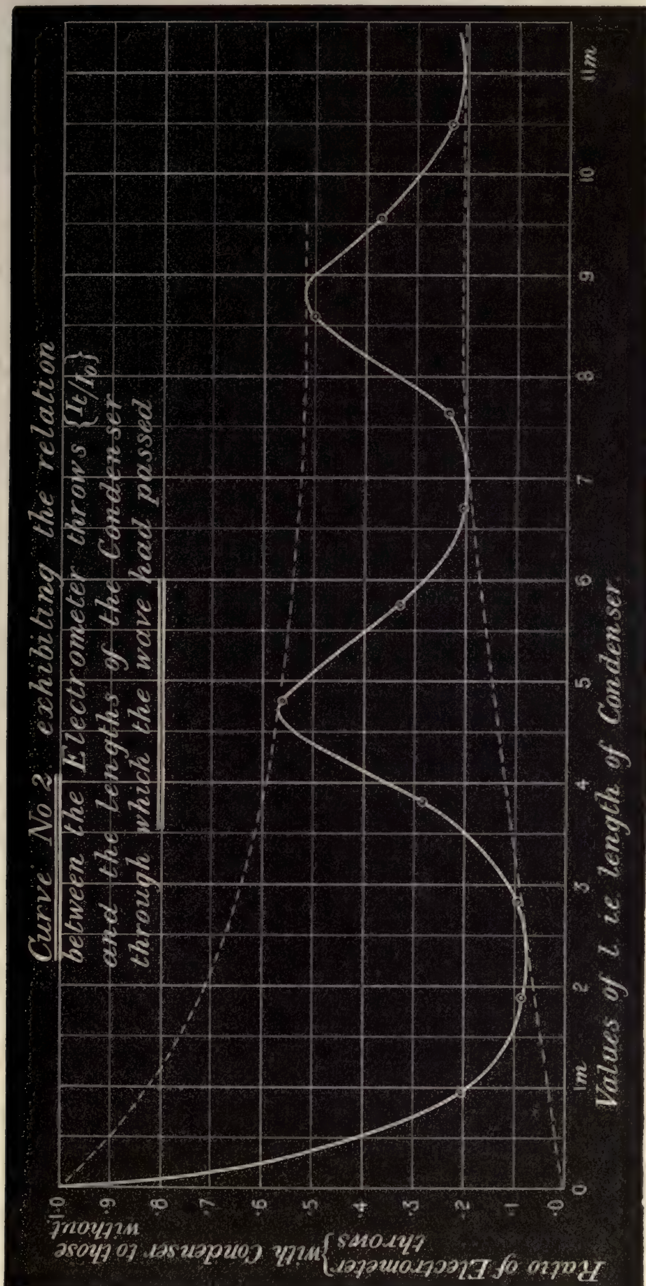
whence

$$b = 0{\cdot}8,$$

by equation (13).

* For this suggestion and for much valuable help in checking mathematical working and results I am indebted to Mr. G. Udny Yule.


Curve No 1 exhibiting the relation
between It/I0 and l from theory
see Equations (18)
The Constants used in this Curve are:
Line with which the Limits of the Curve ultimately coincide
Values of l, i.e. lengths of condenser
Values of It/I0
λ/4
λ/2
3/4 λ
λ
5/4 λ
1m
12m


Curve No 2 exhibiting the relation
between the Electrometer throws {It/I0}
and the Lengths of the Condenser
through which the wave had passed
Values of L i.e. length of Condenser
Ratio of Electrometer throws with Condenser to those without

22. *The Experiment.*—In the experiments performed hitherto, I have made the abnormal part of the conductor by hanging upon the wires of the long secondary sheets of tinfoil 32 cm. deep, the length varying up to 10·5 m. Several observations of the electrometer throws are taken without the condenser, several with the condenser 1 m. long, several with the condenser 2 m. long, and so forth; check observations being taken while the condenser is being shortened again. Curve No. 2 is plotted with condenser lengths as abscissæ, and electrometer throws as ordinates; these latter, however, being reduced to the scale, electrometer throws without condenser equals unity. They thus compare with the values of I_t/I_0 in curve No. 1. The wave-length used was $\lambda_1 = 9$ m.

23. It is seen that the experimental curve thus obtained agrees in its general form with that plotted from theoretical considerations. Exact coincidence of theory and experiment cannot at this stage be expected. I have, accordingly, made no attempt to plot a curve from equation (18) with values of the constants which profess to exactly represent those involved in the experiment.

24. I am aware of two chief sources of disturbances in the experimental conditions, but have already shown that they are not of such order as to invalidate the above results, which, therefore, hold good as first approximations.

25. As the present paper is only a preliminary one, intended to give an outline of the theory and experiment, I will not now enlarge upon the topic of disturbances. I am still engaged on these interference phenomena, and hope to publish a full account of the results next session.

In conclusion, I wish to express my great indebtedness to Professor Hertz, both for first directing my attention to the subject of these reflections, and also for his invaluable advice in the course of the work.

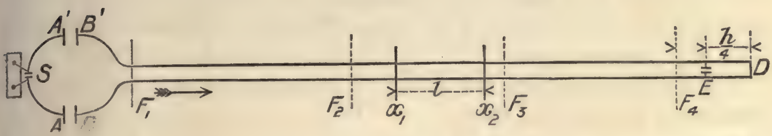
VI. "On Interference Phenomena in Electric Waves passing through different Thicknesses of Electrolyte." By G. Udny Yule. Communicated by Professor G. Carey Foster, F.R.S. Received May 31, 1893.

In the spring of 1889 Professor J. J. Thomson published* a description of some experiments made by him for comparing the resistances of electrolytes to the passage of very rapidly alternating currents, the method consisting in comparing the thicknesses of layers of different electrolytes which were equally opaque to Hertzian radiation. During

* 'Roy. Soc. Proc.,' vol. 45, p. 269, 1889.

last winter I made trial of an arrangement identical in principle but more completely analogous to Hughes' induction balance. The method seemed, however, to offer several difficulties and disadvantages, and finally I adopted another, also, one may say, analogous to Professor Thomson's, inasmuch as it measures transparencies, but in outward appearance completely different from his.

FIG. 1.



Let ASA′ be a Hertz exciter, and B, B′ secondary conductors similar to the primary from which a pair of long wires, stretched parallel to each other, are led off for a considerable distance. One may regard the wires simply as guides for the radiation, which then travels straight up the space between them. If we run these wires for a certain length, l, through an electrolyte, the radiation will have to traverse this and will be partly absorbed. If an electrometer be connected at E, a quarter wave-length from the bridge at the end of the wires, readings taken with various thicknesses of electrolyte should, according to my expectations, give a logarithmic curve, from which the specific resistance would be at once calculable.

The actual dimensions of the exciter, &c., erected were the same as those used by Bjerknes.*

A, A′, B, B′ circular zinc plates, diameter	40 cm.
Distance from A to B	30 ,,
Length of wire ASA′ (2 mm. diameter)	200 ,,
Wave-length, λ	900 ,,

The wires B, F, D, about 1 mm. diameter, were spanned 6 cm. apart. If these wires be made too short, a wave-train emitted from B, B′ may reach the electrolyte x_1, or the bridge D, be reflected, and return to B before the primary has practically done oscillating. If this occur, the state of the secondary may affect the primary as in an alternate current transformer. If, however, Bx_1 be made longer than half the effective length of the wave-train, the reflected waves will not reach B until the primary oscillations have practically come to rest, and under these circumstances the latter will know nothing about any alternations in the secondary at or beyond x_1. This reaction of the secondary on the primary had been first noticed, and to a serious

* 'Wiedemann's Annalen,' vol. 44, p. 513, 1891.

extent, by Herr J. Ritter von Geitler* with an exciter of the type used by Blondlot.†

In the actual apparatus the wires were at F_1 run out through a window in a loop of about 50 m. circumference round the laboratory garden. They re-entered the room at F_2 and were then run vertically through the vessel for containing the electrolyte. The circuit was completed by another loop, F_3F_4, 50 m. long, round the garden, re-entering the room at F_4, connecting to the electrometer at E, and bridged at D, 2·25 m. $= \frac{1}{4}\lambda$ from the electrometer. According to the researches of Bjerknes (*loc. cit.*) these dimensions should be sufficient, with the present apparatus, to prevent any sensible reaction.

The electrometer was the same one as that used by Bjerknes in his researches in the same laboratory. It is a simple quadrant electrometer with only one pair of quadrants and an uncharged aluminium needle of the usual shape suspended by a quartz fibre. One quadrant is connected to each wire. The needle taking no account of sign, elongations are simply proportional to the time integral of the energy: first throws, not steady deflections, are read.

Various glass jars were used for holding the electrolyte. The wires were run vertically through holes drilled in the bottom of the jar, into which they were cemented.

Several trials were made of this apparatus with dilute solutions of copper sulphate. Readings were taken in pairs alternately, with no solution in the jar and with some given thickness; usually about ten readings at each point. The ratio of the transmitted intensities so obtained was determined for several points and plotted as a curve. Some 5 or 6 cm. of electrolyte was the maximum thickness that could be used in these first experiments. The curves so obtained for these badly-conducting solutions always differed sensibly from the logarithmic, and the more so the more the solution was diluted. If the mean log. dec. over the whole thickness was taken, the corresponding value of the specific conductivity appeared extremely high.

It appeared likely that these irregularities might be due to interference effects analogous to Newton's rings (by transmission), or the phenomena of "thin plates," particularly in view of the results obtained just previously by Mr. E. H. Barton in the same laboratory. I consequently desired to investigate for such interference phenomena over as great a thickness of electrolyte as the absorption would permit of using. Distilled water offered itself naturally as the best electrolyte for this purpose.

For the containing vessel a glass cylinder 114 cm. high was used; the internal diameter varied somewhat, but was about 12 cm. at the narrowest.

* Doctor-Dissertation, Bonn, Jan., 1893, p. 22.

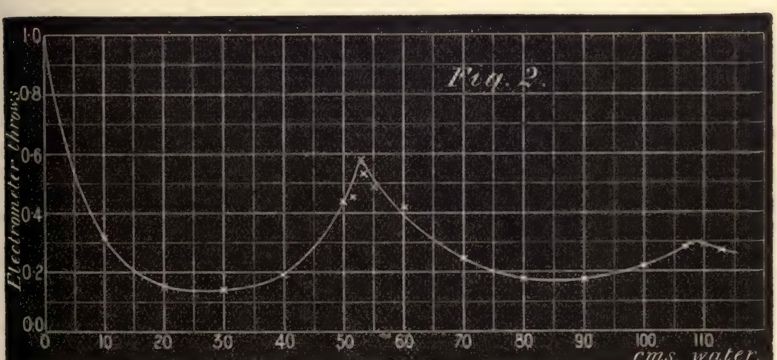
† 'Compt. Rend.,' vol. 114, p. 283, Feb., 1892.

With this apparatus a series of observations were made for various thicknesses of distilled water. To cover, as far as possible, irregularities in sparking, readings were now taken in pairs alternately at the point to be determined and some other point taken for the time as the standard; it would have caused too great delay, and consequent irregularity in the effectiveness of the sparks, were all the water to be siphoned out between each pair of readings. As before, ten or twelve readings were usually taken at each point. The throw obtained with no liquid was also always taken as unity.

As a specimen of the usual spark variations, the following series of readings for the determination of the throw with 55 cm. water with reference to 40 cm. will serve. The series is taken quite at random from the others.

40 cm.		55 cm.
4·6	....	11·4
4·9	....	11·4
5·0	....	11·0
4·2	....	11·9
4·3	....	11·5
3·9	....	11·2
4·0	....	11·6
4·3	....	11·4
4·6	....	10·4
4·4	....	11·2
4·5	....	10·4
4·6	....	10·0

The readings are grouped separately, but it will be understood that they were taken in pairs alternately.



The complete results are given in the curve (Fig. 2). It is seen that for such a poor conductor as distilled water the interference

completely masks the absorption effects. The intensity of the transmitted ray does *not* steadily decrease; on the contrary, far more may be transmitted through a thick than through a thin layer of the absorbent medium. The transmission follows the same general law as for light with a thin plate; we are, in fact, dealing with a "thin" plate—a plate whose thickness is comparable with the wave-length. The intensity of the transmitted ray is a minimum for a plate $\frac{1}{4}\lambda$ thick, a maximum for $\frac{1}{2}\lambda$ thick, a minimum again for $\frac{3}{4}\lambda$, and so on.

The points on the curve round the maximum at $\frac{1}{2}\lambda$ are somewhat irregular, and the two maxima do not absolutely agree. Taking the mean, we may say the wave-lengths in air and water are respectively:—

$$\lambda_a = 900. \qquad \lambda_w = 108 \text{ cm.}$$

This gives us for the coefficient of refraction and the dielectric constant—

$$n = 8{\cdot}33. \qquad \kappa = 69{\cdot}5.$$

The following are the values of κ found by previous investigators, all that are known to me:—

Method used.	Authority.	κ.
Alternated currents ..	Heerwagen*	79·56
	Rosa†	75·70
	Rosa‡	70·C0
Ruhmkorff coil......	Cohn and Arons§	76·00
	Tereschin‖	83·80
Hertz oscillations	Cohn¶	73·50
	Ellinger**	81·00
	Itschegtiaeff††	1·75

Excluding the Russian physicist as a negligible minority, it will be seen that my value of κ is somewhat low. The cause may lie in

* 'Wied. Ann.,' vol. 48, p. 35, 1893.
† 'Phil. Mag.,' vol. 31, p. 200, 1891.
‡ *Ibid.*, vol. 34, p. 344, 1892.
§ 'Wied. Ann.,' vol. 33, p. 13, 1888.
‖ *Ibid.*, vol. 36, p. 792, 1889.
¶ *Ibid.*, vol. 45, p. 370, 1892.
** *Ibid.*, vol. 46, p. 513, 1892.
†† 'Phil. Mag.,' vol. 34, p. 388, 1892.

the fact that not the whole of the field surrounding the wires lies in the water.

The uncertainty due to this stray field might be easily avoided in one way, namely, by making one wire into a tube surrounding the other and using this tube also as the jar for the electrolyte. This was, in fact, the arrangement originally intended to be adopted. Several disadvantages attended it, however, and led to its final rejection in favour of the simple wires and glass jar. First, such a condenser reflects under all circumstances a considerable portion of the incident energy.* Secondly, the variation of the position of the top surface of the electrolyte relatively to the top of the jar would introduce fresh interference phenomena. This appeared directly from the work of Mr. Barton to which I have already had occasion to refer. Lastly, the large surface of metal in contact with the liquid would render distilled water rapidly impure.

This investigation was carried out in the Physical Institute of the University of Bonn. I desire particularly to express my thanks to Professor Hertz for his most useful advice and suggestions.

VII. "On the Ratio of the Specific Heats of the Paraffins and their Monohalogen Derivatives." By J. W. CAPSTICK, M.Sc. (Vict.), B.A. (Camb.), Scholar and Coutts-Trotter Student of Trinity College, Cambridge. Communicated by Professor J. J. THOMSON, F.R.S. Received May 25, 1893.

(Abstract.)

The experiments were undertaken to find whether the internal energy of the molecules of organic gases, as deduced from the ratio of the specific heats, showed any regularities corresponding to the chemical resemblances symbolised by the graphic formulæ.

The paraffins and their monohalogen derivatives are very suitable for the purpose, as their chemical relations to each other are simple, they are easily volatile, and are stable enough to be unaffected by ordinary purifying agents.

From the ratio of the specific heats we can calculate the relative rates of increase of the internal energy and the energy of translation of the molecules per degree rise of temperature, and, the aim of the experiments being to compare the rates of increase of the internal energy of different gases, it was decided to keep the translational energy constant by working at a constant temperature. Consequently the determinations were all made at the temperature of the room.

The ratio of the specific heats was calculated from the velocity of

* J. Ritter von Geitler, Doctor-Dissertation, Bonn, Jan., 1893.

sound in the gases. This was determined by Kundt's method, using a double-ended form of apparatus, in all essential features the same as that described in 'Pogg. Ann.,' vol. 135. The tube in which the dust figures were made was 125 cm. long and 26 mm. in diameter, which Kundt showed to be great enough to avoid any lowering of the velocity of sound from the influence of the walls of the tube.

Lycopodium was used for forming the figures in the hydrocarbons and in methyl and ethyl chlorides, but in the heavier gases it became sticky, and would not move readily, so for these silica was used.

To measure the figures a piece of apparatus was constructed, consisting of a pair of parallel platinum wires, carried by a framework which slides along a steel scale graduated to millimetres. The tube was placed on V-shaped supports, parallel to the scale, and between the wires, which were so adjusted that their plane passed through the centre of the nodes. The position of the framework was then read on the scale, tenths of a millimetre being estimated with the help of a lens. With figures of average quality the setting of the wires could be repeated so as to agree within two or three tenths of a millimetre. The greatest divergence of the observed length of any one figure from the mean of the series was usually about five or six tenths of a millimetre.

The calculation of the ratio of the specific heats from the velocity of sound requires the density of the gas to be known, a circumstance which makes the method very sensitive to small amounts of impurity in the material.

Regnault's value of the density was used for methane, and every precaution was taken to secure pure gas. Two methods of preparation were used, Gladstone and Tribe's, by the action of the copper-zinc couple on methyl iodide and alcohol, and Frankland's, by the action of zinc methyl on water. After each experiment an analysis of the gas was made to test its purity and to determine the amount of air present, for which a correction was made.

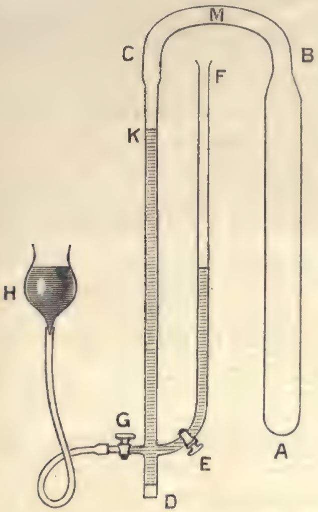
The ethane was prepared by the action of zinc ethyl on water, and for it the theoretical density calculated from the molecular weight was used.

For the preparation of propane, isopropyl iodide was reduced by zinc and hydrochloric acid, and the gas was freed from air by liquefaction in a freezing mixture of ether and solid carbonic acid, after passing through fuming sulphuric acid and potash, and over 30 grams of palladium.

Methyl and ethyl chlorides were prepared in the usual way, by passing hydrochloric acid into a boiling solution of zinc chloride in the corresponding alcohol, and purified by redistillation through suitable reagents. All the rest of the compounds were purchased from Kahlbaum, and were dried and fractionated before being used.

The vapour densities of propane and all the halogen compounds were determined at various pressures for the material as it was used in the velocity of sound experiment, thus avoiding to a great extent any error arising from impurity.

FIG. 1.



The apparatus used for this purpose is shown in fig. 1. Before joining the parts together, the tube CD is calibrated, and after it has been attached, but before the side tubes are fixed on, the volume of the whole is determined by filling with water and weighing. Then, from the calibration of CD, the volume is known between A and a file mark at K.

A weighed quantity of the liquefied gas whose vapour density is required is sealed up in a small tube with capillary ends, and introduced at D, and, by inclining the apparatus, is made to slide over the bend and rest at M. The end D is then closed with a cork, and the apparatus exhausted through the three-way tap G, on the completion of which operation mercury is allowed to flow in from the reservoir H, and the tap E is opened. The difference of the levels of the mercury in the two tubes is read with a cathetometer, and, subtracted from the height of the barometer, gives the pressure of the residual air. On tilting the apparatus, the tube slides over into the wider part, and the end breaks off, allowing the liquid to evaporate. By reading the levels again we get the pressure of the gas, and knowing its weight and volume, we have all the materials required for calculating its specific gravity.

In the experiments the reservoir was always adjusted so that the level of the mercury in CD stood near the mark at K, thus simplifying the calculations a little.

The apparatus was found to give values of the vapour density concordant to about one part in 1000.

The formula used in calculating the ratio of the specific heats was

$$\gamma = 1{\cdot}408 \times \rho \times \left(\frac{l}{l'}\right)^2 \left(1 + \frac{1}{p}\frac{d}{dv}(pv)\right),$$

the last factor being added to the ordinary formula to correct for the divergence of the gas from Boyle's Law.

The correction is obtained at once by putting in the equation $u^2 = -\gamma v^2 \left(\frac{dp}{dv}\right)_t$ the value of $\left(\frac{dp}{dv}\right)_t$ given by $\left(\frac{dpv}{dv}\right)_t = p + v\left(\frac{dp}{dv}\right)_t$.

From the vapour density determinations a curve is constructed giving pv in terms of v, and the slope of this curve at any point gives the value of $\frac{d}{dv}(pv)$ in arbitrary units. Dividing by the corresponding value of p in the same units, we obtain the amount of the correction.

The correction increases the ratio of the specific heats by from 1 to 2 per cent. in most cases.

Observations varying in number from three to nine were made on each gas, the extreme range of the values being 2 per cent. for marsh gas, $1\frac{1}{2}$ per cent. for methyl iodide, and 1 per cent., or less, for the rest.

The mean values are shown in the following table:—

Methane	CH_4	1·313
Methyl chloride	CH_3Cl	1·279
Methyl bromide	CH_3Br	1·274
Methyl iodide	CH_3I	1·286
Ethane	C_2H_6	1·182
Ethyl chloride	C_2H_5Cl	1·187
Ethyl bromide	C_2H_5Br	1·188
Propane	C_3H_8	1·130
Normal propyl chloride	nC_3H_7Cl	1·126
Isopropyl chloride	iC_3H_7Cl	1·127
Isopropyl bromide	iC_3H_7Br	1·131

From this table we have the interesting result that the gases fall into four groups, the members of any one group having within the limits of experimental error the same ratio of the specific heats.

These groups are—

I. Methane.
II. The three methyl compounds.
III. Ethane and its derivatives.
IV. Propane and its derivatives.

If the members of a group have the same ratio of the specific heats, we know, from a well-known equation in the kinetic theory of gases, that the ratio of the internal energy absorbed by the molecule to the total energy absorbed, per degree rise of temperature, is the same for all. Hence we have the result that, with the single exception of marsh gas, the compounds with similar formulæ have the same energy-absorbing power, a result which supplies a link of a kind much needed to connect the graphic formula of a gas with the dynamical properties of its molecules.

From the conclusion we have reached, it follows with a high degree of probability that the atoms which can be interchanged without effect on the ratio of the specific heats have themselves the same energy-absorbing power, their mass and other special peculiarities being of no consequence. Further, the anomalous behaviour of methane confirms what was clear from previous determinations, namely, that the number of atoms in the molecule is not in itself sufficient to fix the distribution of energy, and suggests that perhaps the configuration is the sole determining cause.

If this is so, it follows that ethane and propane have the same configuration as their monohalogen derivatives, but that methane differs from the methyl compounds, a conclusion that in no way conflicts with the symmetry of the graphic formulæ of methane and its derivatives, for this is a symmetry of reactions, not of form.

VIII. "On Operators in Physical Mathematics. Part II." By Oliver Heaviside, F.R.S. Received June 8, 1893.

Algebraical Harmonization of the Forms of the Fundamental Bessel Function in Ascending and Descending Series by means of the Generalized Exponential.

27. As promised in § 22, Part I ('Roy. Soc. Proc.,' vol. 52, p. 504), I will now first show how the formulæ for the Fourier-Bessel function in rising and descending powers of the variable may be algebraically harmonized, without analytical operations. The algebraical conversion is to be effected by means of the generalized exponential theorem, § 20. It was, indeed, used in § 22 to generalize the ascending form of the function in question; but that use was analytical. At present it is to be algebraical only. Thus, let

$$A = 1+\frac{x^2}{2^2}+\frac{x^4}{2^2 4^2}+\frac{x^6}{2^2 4^2 6^2}+\ldots\ldots, \tag{1}$$

$$B = \frac{2}{\pi}\left(x+\frac{x^3}{3^2}+\frac{x^5}{3^2 5^2}+\ldots\ldots\right)+\frac{2}{\pi}\left(\frac{1}{x}+\frac{1^2}{x^3}+\frac{1^2 3^2}{x^5}+\ldots\ldots\right), \tag{2}$$

$$C = \frac{\epsilon^x}{(2\pi x)^{\frac{1}{2}}}\left(1+\frac{1^2}{8x}+\frac{1^2 3^2}{\underline{|2}(8x)^2}+\frac{1^2 3^2 5^2}{\underline{|3}(8x)^3}+\ldots\ldots\right). \tag{3}$$

Here A is the usual form of the Fourier-Bessel function (or, rather, the function $I_0(x)$ instead of the oscillating function $J_0(x)$, whose theory is less easy), or the first solution in rising powers of x^2 of the differential equation

$$(\nabla^2+x^{-1}\nabla)u = u, \tag{4}$$

as in (71), (72), Part I. Also, B is a particular case, viz., (78), Part I, of the generalization of the same series, (77), Part I, using the odd powers of x, and going both ways, in order to complete the series. And C is an equivalent form of the same function in a descending series, (31), Part I, obtained analytically, before the subject of generalized differentiation was introduced. The analytical transformation from A to C was considered in § 14. The present question is, what relation does C bear to A and B algebraically? It cannot be algebraically identical with either of them alone, on account of the radical in C. We may, however, eliminate the radical by employing the particular case of the generalized exponential that will introduce the radical anew. Thus, (63), (64), Part I,

$$\epsilon^x = \ldots\ldots+\frac{x^{-\frac{3}{2}}}{\underline{|-\tfrac{3}{2}}}+\frac{x^{-\frac{1}{2}}}{\underline{|-\tfrac{1}{2}}}+\frac{x^{\frac{1}{2}}}{\underline{|\tfrac{1}{2}}}+\frac{x^{\frac{3}{2}}}{\underline{|\tfrac{3}{2}}}+\ldots\ldots. \tag{5}$$

If we use this in (3), and carry out the multiplications, we obtain a series in integral powers of x, positive and negative; thus,

$$\begin{aligned} C = \frac{1}{(2\pi)^{\frac{1}{2}}}\Bigg[&\frac{1}{\underline{|\tfrac{1}{2}}}+\frac{(\frac{1}{2})^2}{2\underline{|\tfrac{3}{2}}}+\frac{(\frac{1}{2}\cdot\frac{3}{2})^2}{2^2\underline{|2}\,\underline{|\tfrac{5}{2}}}+\frac{(\frac{1}{2}\cdot\frac{3}{2}\cdot\frac{5}{2})^2}{2^3\underline{|3}\,\underline{|\tfrac{7}{2}}}+\ldots\ldots \\ &+x\left(\frac{1}{\underline{|\tfrac{3}{2}}}+\frac{(\frac{1}{2})^2}{2\underline{|\tfrac{5}{2}}}+\frac{(\frac{1}{2}\cdot\frac{3}{2})^2}{2^2\underline{|2}\,\underline{|\tfrac{7}{2}}}+\ldots\ldots\right) \\ &+x^2\left(\frac{1}{\underline{|\tfrac{5}{2}}}+\frac{(\frac{1}{2})^2}{2\underline{|\tfrac{7}{2}}}+\frac{(\frac{1}{2}\cdot\frac{3}{2})^2}{2^2\underline{|2}\,\underline{|\tfrac{9}{2}}}+\ldots\ldots\right)+\ldots\ldots \\ &+x^{-1}\left(\frac{1}{\underline{|-\tfrac{1}{2}}}+\frac{(\frac{1}{2})^2}{2\underline{|\tfrac{1}{2}}}+\frac{(\frac{1}{2}\cdot\frac{3}{2})^2}{2^2\underline{|2}\,\underline{|\tfrac{3}{2}}}+\ldots\ldots\right)+\ldots\ldots\Bigg]. \end{aligned} \tag{6}$$

28. Now B involves all the odd powers of x, whilst A involves only the even positive powers. But the terms involving even negative

powers in A are zero, if we follow the law of the coefficients. So A is also complete, and C must be some combination of the series A and B. In fact, if we assume that

$$u = a_0 + a_1 x + a_2 x^2 + \dots + b_1 x^{-1} + b_2 x^{-2} + \dots \qquad (7)$$

is a solution of the characteristic (4), and insert it therein, to find the law of the coefficients in the usual manner, we find that the even b's are zero, whilst the even a's are connected in one way, and the odd a's and even b's are independently connected in another way. This makes

$$u = a\mathrm{A} + b\mathrm{B}, \qquad (8)$$

where a and b are independent multipliers. Now, judging from common experience with this rule-of-thumb method of constructing solutions of differential equations, we might hastily conclude that A and B represented the two independent solutions of the characteristic. Here, however, we know (analytically) that they are not independent, but are equivalent. Therefore

$$\mathrm{C} = a\mathrm{A} + b\mathrm{B}, \qquad (9)$$

where the sum of a and b is unity. It only remains to find the value of a. This is easily obtainable, because the separate series in (6) are rapidly convergent. But we need only employ the first series, viz., to find the coefficient of x^0. Thus, the first line of (6) gives

$$a = \frac{\sqrt{2}}{\pi}\left\{1 + \tfrac{1}{12}(1 + \tfrac{9}{40}(1 + \tfrac{25}{84}(1 + \tfrac{49}{144}(1 + \dots\right\}$$

$$= \frac{1 \cdot 1106}{2 \cdot 2214} = 0 \cdot 5. \qquad (10)$$

We see, therefore, that the series C is algebraically identical with half the sum of the series A and B.

To further verify, we see that the coefficient of x in (6) should be $2/\pi$ times that of x^0. This requires

$$\frac{2}{\pi} \times 1 \cdot 1106 = \tfrac{2}{3}(1 + \tfrac{1}{20}(1 + \tfrac{9}{56}(1 + \tfrac{25}{108}(1 + \dots,$$

or

$$0 \cdot 7068 = 0 \cdot 7067,$$

which is also close. Similarly, from the x^2 series we require

$$\frac{1 \cdot 1106}{4} = \tfrac{4}{15}(1 + \tfrac{1}{28}(1 + \tfrac{1}{8}(1 + \tfrac{25}{132}(1 + \dots,$$

or

$$0 \cdot 277 = 0 \cdot 277.$$

The numerical tests in this example are perfectly satisfactory; and if the numerical meaning of a divergent series could be always as easily fixed, it would considerably facilitate the investigation of the subject.

Condensed Generalized Notation. Generalization of the Descending Series for the Bessel Function through the Generalized Binomial Theorem.

29. Since the series A and B are particular cases of the general formula (77), Part I, or of

$$u = \Sigma \frac{\left(\frac{x^2}{4}\right)^r}{(\underline{|r})^2}, \tag{11}$$

by taking $r = 0$ and $r = \frac{1}{2}$ respectively, it may be desirable to find the general formula of which the series C is a particular case. Notice, in passing, the shorter notation employed in (11). It is certainly easier to see the meaning of a series by inspecting the written-out formula containing several terms, when one is not familiar with the kind of series concerned. As soon, however, as one gets used to the kind of formula, the writing out of several terms becomes first needless, and then tiresome. The short form (11) is then sufficient. One term only is written, with the summation sign before it. The other terms are got by changing r with unit step always, and both ways. The value of r is arbitrary, though of course it should have the same value in every term so far as the fractional part is concerned, so that, in (11), r may be changed to any other number without affecting its truth. Similarly, the exponential formula may be written

$$\epsilon^x = \Sigma \frac{x^r}{\underline{|r}}, \tag{12}$$

with r arbitrary and unit step.

Now, to find the generalized formula wanted, we have, by (25), Part I,

$$I_0(x) = \epsilon^x (1+2\nabla^{-1})^{-\frac{1}{2}}. \tag{13}$$

Expand this according to the particular form of the binomial theorem got by taking $n = -\frac{1}{2}$ in (84), Part I, leaving m arbitrary. Or writing that general formula thus:—

$$\frac{(1+x)^n}{\underline{|n}} = \Sigma \frac{x^r}{\underline{|r}\,\underline{|n-r}}, \tag{14}$$

which is compact and intelligible, according to the above explanation, take $n = -\frac{1}{2}$, and write $2\nabla^{-1}$ in place of x. This makes

$$\frac{(1+2\nabla^{-1})^{-\frac{1}{2}}}{\underline{|-\tfrac{1}{2}}} = \Sigma \frac{(2\nabla^{-1})^r}{\underline{|r}\,\underline{|-\tfrac{1}{2}-r}}. \tag{15}$$

Effect the integration, and we obtain immediately

$$= \Sigma \frac{(2x)^r}{(\underline{|r})^2\,\underline{|-\tfrac{1}{2}-r}}; \tag{16}$$

and therefore, by (13),

$$\mathrm{I}_0(x) = \epsilon^x \underline{|-\tfrac{1}{2}}\,\Sigma \frac{(2x)^r}{(\underline{|r})^2\,\underline{|-\tfrac{1}{2}-r}}. \tag{17}$$

Here we see the great convenience in actual work of the condensed notation. At the same time, it is desirable to expand sometimes and see what the developed formula looks like. We then take the written term as a central basis, making it a factor of all the rest. Thus,

$$\begin{aligned}\mathrm{I}_0(x) = \frac{(2x)^r \epsilon^x \underline{|-\tfrac{1}{2}}}{\underline{|-\tfrac{1}{2}-r}\,(\underline{|r})^2} \Big\{ 1 + \frac{2x(-\frac{1}{2}-r)}{(r+1)^2}\Big(1+\frac{2x(-\frac{3}{2}-r)}{(r+2)^2}\Big(1+\ldots \\ + \frac{(2x)^{-1}r^2}{(\frac{1}{2}-r)}\Big(1+\frac{(2x)^{-1}(r-1)^2}{(\frac{3}{2}-r)}\Big(1+\ldots \Big\}.\end{aligned} \tag{18}$$

30. Take $r = 0$ in this, and we have

$$\mathrm{I}_0(x) = \epsilon^x \Big(1 - x\Big(1-\frac{3x}{2^2}\Big(1-\frac{5x}{3^2}\Big(1-\ldots, \tag{19}$$

which is the same as (20), Part I, noting that $\frac{1}{2}at$ there is x here. But of course the exponential factor is now of no service, the ordinary series A, equation (1) above, being the practical formula when x is small.

Take $r = -\frac{1}{2}$ in (18), and we obtain

$$\mathrm{I}_0(x) = \frac{\epsilon^x}{(2\pi x)^{\frac{1}{2}}}\Big\{1+\frac{1^2}{8x}\Big(1+\frac{3^2}{2\,.\,8x}\Big(1+\frac{5^2}{3\,.\,8x}\Big(1+\ldots, \tag{20}$$

which is the formula C, equation (3) above, the practical formula when x is bigger than is suitable for rapid calculation by A. Observe that these are the extreme cases, for the whole of the second line in (18) goes out to make (19), and the whole of the first line, excepting the first term, goes out to make (20). On the other hand, it frequently happens that extreme cases of a generalized formula are numerically uninterpretable.

To convert (18) to the form $a\mathrm{A}+b\mathrm{B}$ algebraically, we may use the exponential expansion in the form (12), but with r negatived, thus,

$$\epsilon^x = \Sigma \frac{x^{-r}}{\underline{|-r}}. \tag{21}$$

Employing this in (18), we can reduce the series to one containing integral powers only. The coefficient of x^0 is made to be

$$\Sigma \frac{2^r \underline{|-\frac{1}{2}}}{(\underline{|r})^2 \underline{|-\frac{1}{2}-r} \, \underline{|-r}} . \tag{22}$$

That this reduces correctly to a convergent series summing up to $\frac{1}{2}$, when $r=-\frac{1}{2}$, may be anticipated and verified. Also, that when $r=0$ we obtain unity is sufficiently evident. In these conclusions we merely corroborate the preceding. But I have not been able to reduce (22) to a simple formula showing plainly in what ratio the formulæ A and B are involved when r has any other values than 0 and $\frac{1}{2}$ (or, any integral value, and the same *plus* $\frac{1}{2}$).

The Extreme Forms of the Binomial Theorem. Obscurities.

31. There are some peculiarities about the extreme forms of the binomial theorem when the exponent is negative unity (or a negative integer) which deserve to be noticed, because they are concerned in failures, or apparent failures, which occur in derived formulæ. These peculiarities are connected with the vanishing of the inverse factorial for any negative integral value of the argument. Thus, in

$$\frac{(1+x)^n}{\underline{|n}} = \Sigma \frac{x^r}{\underline{|r} \, \underline{|n-r}} , \tag{23}$$

take $n=-1$. We obtain

$$\frac{(1+x)^{-1}}{\underline{|-1}} = \frac{x^r}{\underline{|r} \, \underline{|-1-r}} \Big\{ (1-x+x^2-x^3+\dots\dots) \\ -(x^{-1}-x^{-2}+x^{-3}-x^{-4}+\dots\dots) \Big\} . \tag{24}$$

Now, on the left side we have the vanishing factor $(\underline{|-1})^{-1}$. So, on the right side, the quantity in the big brackets should generally vanish. This asserts that

$$1-x+x^2-x^3+\dots\dots = x^{-1}-x^{-2}+x^{-3}-x^{-4}+\dots\dots , \tag{25}$$

where on the left side we have the result of dividing 1 by $1+x$, and, on the right, the result of dividing 1 by $x+1$, or x^{-1} by $1+x^{-1}$. These series are the extreme forms of the expansion of $(1+x)^{-1}$ by the ordinary binomial theorem, and they are asserted to be algebraically equivalent, although the numerical equivalence, which is sometimes recognisable, is often scarcely imaginable.

But observe that if we choose $r=0$ as well, we have a nullifying factor on the right side also of (24). It is apparently the same as

the other, and could be removed from both sides if it were finite. It must not, however, be removed from (24). What is asserted is that $0\times(1+x)^{-1}=0\times 0$, where the first 0 on the right is $(\underline{|-1})^{-1}$, and also the 0 on the left.

Again, if we put $r=0$ first in (23), making

$$\frac{(1+x)^n}{\underline{|n}}=\frac{1}{\underline{|n}}+\frac{x}{\underline{|1}\,\underline{|n-1}}+\dots+\frac{x^{-1}}{\underline{|n+1}\,\underline{|-1}}+\frac{x^{-2}}{\underline{|n+2}\,\underline{|-2}}+\dots, \tag{26}$$

and then put $n=-1$, we get $0=0$. But if we multiply (26) by $\underline{|n}$, making

$$(1+x)^n=1+nx+\frac{n(n-1)}{\underline{|2}}x^2+\dots$$

$$+\frac{x^{-1}}{(n+1)\,\underline{|-1}}+\frac{x^{-2}}{(n+1)(n+2)\,\underline{|-2}}+\dots, \tag{27}$$

we see that the descending series vanishes when n is any negative integer. That is, it is asserted that

$$(1+x)^n=1+nx+\frac{n(n-1)}{1\,.\,2}x^2+\dots, \tag{28}$$

unless n is negatively integral. But when it is a negative integer there are additional terms, though always in indeterminate form; for instance, $\infty\times 0$ when $n=-1$ and x is finite. It would appear, however, that the value is zero, because there is every reason to think (28) correct (as a particular form) in the limit.

On the other hand, if we multiply (26) by $\underline{|-1}$, and so make it cancel the $\underline{|-1}$, $\underline{|-2}$, &c., in the denominators, we get, when n is -1,

$$(1+x)^{-1}=1-x+x^2-\dots+x^{-1}-x^{-2}+x^{-3}-\dots, \tag{29}$$

which is quite inadmissible, since the right member is the sum of two series previously found to be equivalent to one another, and to the left member. The right member is therefore twice as great as the left.

Improved Statement of the Binomial Theorem with Integral Negative Index.

32. A consideration of the above obscurities suggests the following way of avoiding them. We should recognize that the zeros $(\underline{|n})^{-1}$ and r, when we take $n=-1$ and $r=0$, are independent, and may have any ratio we please. Thus, first put $n=-1+s$ in (23), making

$$\frac{(1+x)^{-1+s}}{\underline{|-1+s}} = \frac{x^r}{\underline{|r}} \frac{1}{\underline{|-1+s-r}} \left\{ 1 + \frac{-1+s-r}{r+1} x \left(1 + \frac{-2+s-r}{r+2} x \left(1 + \ldots \right.\right.\right.$$

$$\left. + \frac{r}{s-r} x^{-1} \left(1 + \frac{r-1}{(s-r)(1+s-r)} x^{-1} \left(1 + \ldots \right.\right.\right\}. \quad (30)$$

This being general, let r and s be both infinitely small, but without any connexion. We know that the rate of increase of the inverse factorial with n is 1 when n is -1. It follows that

$$\frac{1}{\underline{|-1+s}} = s, \qquad \frac{1}{\underline{|-1+s-r}} = s-r. \quad (31)$$

These, used in (30), make it become

$$s(1+x)^{-1+s} = \frac{x^r}{\underline{|r}}(s-r)\left\{ 1 + \frac{-1+s-r}{r+1} x + \ldots \right.$$

$$\left. + \frac{r}{s-r} x^{-1} + \frac{r(r-1)}{(s-r)(1+s-r)} x^{-2} + \ldots \right\}. \quad (32)$$

Ultimately, therefore, we obtain in a clear manner

$$(1+x)^{-1} = \left(1-\frac{r}{s}\right)\left(1-x+x^2-x^3+\ldots\right) + \frac{r}{s}\left(x^{-1}-x^{-2}+x^{-3}-\ldots\right). \quad (33)$$

This seems to be the proper limiting form of the binomial theorem when the index is negative unity. It asserts that the two extreme equivalent forms may be combined in any ratio we please, since r/s may have any value. If $r=0$, we have the ascending series only. If $r=s$, then the descending series only. If $s=2r$, we obtain half their sum. The expansion is indeterminate, but the degree of indeterminateness appears to be merely conditioned by the size of the ratio r/s.

We may also notice that the suppositions that s is infinitely small and r is finite, so that

$$\frac{1}{\underline{|-1+s}} = s, \quad \text{and} \quad \frac{1}{\underline{|-1+s-r}} = \frac{1}{\underline{|-1-r}},$$

used in (30), lead us to

$$(1+x)^{-1} = \frac{x^r}{\underline{|r}} \frac{1}{\underline{|-1-r}} \cdot \frac{1}{s} \left\{ 1-x+x^2-x^3+\ldots \right.$$

$$\left. -x^{-1}+x^{-2}-x^{-3}+\ldots \right\}; \quad (34)$$

that is, the difference of the two extreme equivalent series divided by 0, which is, of course, indeterminate.

Consideration of a more general Operator, $(1+\nabla^{-1})^n$. Suggested Derived Equivalences.

33. Some years since, after noticing first the analytical and then later the numerical equivalence of the different formulæ for the Fourier-Bessel function arising immediately from the operator $(1+\nabla^{-1})^{-\frac{1}{2}}$ by the use of the two extreme forms of the binomial theorem (the only forms then known to me), I endeavoured to extend the results by substituting the operator $(1+\nabla^{-1})^n$, which includes the former, and comparing the extreme forms. Thus, calling u the series in ascending powers of ∇^{-1}, and v the descending series, so that

$$u = 1+n\nabla^{-1}+\frac{n(n-1)}{\underline{|2}}\nabla^{-2}+\ldots., \tag{35}$$

$$v = \nabla^{-n}\left(1+n\nabla+\frac{n(n-1)}{\underline{|2}}\nabla^{2}+\ldots.\right), \tag{36}$$

and integrating (with x^0 for operand, as usual when no operand is written), we obtain

$$u = 1+nx+\frac{n(n-1)}{\underline{|2}\,\underline{|2}}x^2+\frac{n(n-1)(n-2)}{\underline{|3}\,\underline{|3}}x^3+\ldots., \tag{37}$$

$$v = \frac{x^n}{\underline{|n}}\left(1+\frac{n^2}{x}+\frac{n^2(n-1)^2}{x^2\underline{|2}}+\frac{n^2(n-1)^2(n-2)^2}{x^3\underline{|3}}+\ldots.\right); \tag{38}$$

and the suggestion is that these are equivalent. If this equivalence is analytical, and we substitute ∇^{-1} for x and integrate a second time, we obtain

$$1+nx+\frac{n(n-1)}{(\underline{|2})^3}x^2+\frac{n(n-1)(n-2)}{(\underline{|3})^3}x^3+\ldots. \tag{39}$$

$$= \frac{x^n}{(\underline{|n})^2}\left\{1+\frac{n^3}{x}+\frac{n^3(n-1)^3}{x^2\underline{|2}}+\frac{n^3(n-1)^3(n-2)^3}{x^3\underline{|3}}+\ldots.\right\}; \tag{40}$$

and obvious repetitions of the same process lead us to

$$1+nx+\frac{n(n-1)}{(\underline{|2})^m}x^2+\frac{n(n-1)(n-2)}{(\underline{|3})^m}x^3+\ldots. \tag{41}$$

$$= \frac{x^n}{(\underline{|n})^{m-1}}\left\{1+\frac{n^m}{x}+\frac{n^m(n-1)^m}{x^2\underline{|2}}+\frac{n^m(n-1)^m(n-2)^m}{x^3\underline{|3}}+\ldots.\right\}, \tag{42}$$

which are clearly the cases $r = 0$ and $r = n$ of the general expression

$$\Sigma\frac{x^r\underline{|n}}{(\underline{|r})^m\underline{|n-r}}; \tag{43}$$

provided n is not a negative integer, when we know that closer examination is required.

Apparent Failure of Numerical Equivalence in certain Cases.

34. Now, although the equations following (35), (36) (excepting (43)) are deducible from them by the process used immediately and without trouble, there is considerable difficulty in finding out their meaning. Considering (37) and (38), I knew that in the case $n = -\frac{1}{2}$ the equivalence was satisfactory all round, though not very understandable. When n is 0, or integral, it is also satisfactory, for then we have merely a perversion of terms in passing from u to v. But when I tried the case $n = -\frac{1}{4}$, and subjected it to numerical calculation, with the expectation of finding numerical equivalence to the extent permitted by the initial convergence of the divergent series, I found a glaring discrepancy between u and v. Furthermore, on taking $n = -1$, we produce

$$u = \epsilon^{-x}, \tag{44}$$

$$v = \frac{x^{-1}}{\underline{|-1}}\left(1+\frac{1}{x}+\frac{\underline{|2}}{x^2}+\frac{\underline{|3}}{x^3}+\dots\right), \tag{45}$$

which show no sort of numerical equivalence whatever. Similarly, $n = -2$ gives

$$u = 1-2x+\frac{3}{\underline{|2}}x^2-\frac{4}{\underline{|3}}x^3+\dots, \tag{46}$$

$$v = \frac{x^{-2}}{\underline{|-2}}\left(1+\frac{2^2}{x}+\frac{2^2 3^2}{\underline{|2}x^2}+\frac{2^2 3^2 4^2}{\underline{|3}x^3}+\dots\right), \tag{47}$$

which also do not show any numerical equivalence. I was therefore led to think that the equivalence in the case of the Fourier-Bessel function was due to some peculiarity of that function, and it is a fact that the function is the meeting-place of many remarkabilities. The matter was therefore put on one side for the time. But, more recently, independent evidence in other directions showed me that there was no particular reason to expect such a complete failure. And, in fact, on returning to the discrepant calculations relating to $n = -\frac{1}{4}$, I found an important numerical error. When corrected, the results for u and v agreed as fairly as could be expected.

Probable Satisfaction of Numerical Equivalence by Initial Convergence within a certain Range for n, viz., $n = -\frac{1}{2}$ to $+1$.

35. Thus, $n = -\frac{1}{4}$ in (37), (38) produces

$$u = 1 - \frac{x}{4} + \frac{1.5}{\underline{|2}\,\underline{|2}}\left(\frac{x}{4}\right)^2 - \frac{1.5.9}{\underline{|3}\,\underline{|3}}\left(\frac{x}{4}\right)^3 + \ldots, \tag{48}$$

$$v = \frac{x^{-\frac{1}{4}}}{\underline{|-\frac{1}{4}}}\left(1 + \frac{1}{16x} + \frac{1^2 5^2}{\underline{|2}\,(16x)^2} + \frac{1^2 5^2 9^2}{\underline{|3}\,(16x)^3} + \ldots\right). \tag{49}$$

Here take $x = 4$. Then

$$u = 1 - 1(1 - \tfrac{5}{4}(1 - 1(1 - \tfrac{13}{16}(1 - \tfrac{17}{25}(1 - \ldots, \tag{50}$$

$$v = \frac{1}{4^{\frac{1}{4}}\underline{|-\frac{1}{4}}}\left\{1 + \frac{1}{64}\left(1 + \frac{25}{2.64}\left(1 + \frac{81}{3.64}\left(1 + \frac{169}{4.64}\left(1 + \ldots\right.\right.\right.\right.\right. . \tag{51}$$

This was the test case which failed, the error arising from the numerical equality of two consecutive terms, and then, a little later, of another two consecutive terms, which caused a skipping. I now make

$$u = 0{\cdot}5880, \qquad v = \frac{1{\cdot}0216}{4^{\frac{1}{4}}\underline{|-\frac{1}{4}}}.$$

Their equivalence requires that

$$\frac{1}{\underline{|-\frac{1}{4}}} = \frac{0{\cdot}5880 \times 1{\cdot}4142}{1{\cdot}0216} = 0{\cdot}814,$$

which is about right.

When $x = 2$, we have

$$u = 1 - \tfrac{1}{2} + \tfrac{5}{16}(1 - \tfrac{9}{18}(1 - \tfrac{13}{32}(1 - \tfrac{17}{50}(1 - \tfrac{21}{72}(1 - \ldots,$$

$$v = \frac{1}{2^{\frac{1}{4}}\underline{|-\frac{1}{4}}}\left\{1 + \tfrac{1}{32}(1 + \tfrac{25}{64}(1 + \tfrac{81}{96}(1 + \ldots\right\},$$

giving $\qquad u = 0{\cdot}706, \qquad v = \dfrac{1{\cdot}043}{2^{\frac{1}{4}}\underline{|-\frac{1}{4}}};$

which requires $\qquad \dfrac{1}{\underline{|-\frac{1}{4}}} = 0{\cdot}805.$

And when $x = 1$ we have

$$u = 1 - \tfrac{1}{4} + \tfrac{5}{64}(1 - \tfrac{9}{36}(1 - \tfrac{13}{64}(1 - \tfrac{17}{100}(1 - \ldots,$$

$$v = \frac{1}{\underline{|-\frac{1}{4}}}(1 + \tfrac{1}{16}(1 + \tfrac{25}{32}(1 + \tfrac{81}{48}(1 + \ldots;$$

I 2

giving $$u = 0{\cdot}8123, \qquad v = \frac{1{\cdot}0625}{\underline{\lvert -\frac{1}{4}}},$$

requiring $$\frac{1}{\underline{\lvert -\frac{1}{4}}} = 0{\cdot}76.$$

Of course with such a small value of x, we cannot expect more than a very rough agreement, because the convergence of the v series is confined to the first and second terms, and we may expect an error of magnitude of the ratio of the second to the first term.

36. Now take $n = \frac{1}{4}$. We have

$$u = 1 + \frac{x}{4} - \tfrac{3}{4}\left(\frac{x}{4}\right)^2 + \frac{1\,.\,3\,.\,7}{\underline{\lvert 3}\,\underline{\lvert 3}}\left(\frac{x}{4}\right)^3 - \frac{1\,.\,3\,.\,7\,.\,11}{\underline{\lvert 4}\,\underline{\lvert 4}}\left(\frac{x}{4}\right)^4 + \dots, \tag{52}$$

$$v = \frac{x^{\frac{1}{4}}}{\underline{\lvert \frac{1}{4}}}\left\{1 + \frac{1}{16x} + \frac{9}{\underline{\lvert 2}\,(16x)^2} + \frac{3^2\,.\,7^2}{\underline{\lvert 3}\,(16x)^3} + \dots\right\}; \tag{53}$$

and in case of $x = 1$ we have

$$u = 1 + \tfrac{1}{4}(1 - \tfrac{3}{16}(1 - \tfrac{7}{36}(1 - \tfrac{11}{64}(1 - \tfrac{15}{100}(1 - \dots, \tag{54}$$

$$v = \frac{1}{\underline{\lvert \frac{1}{4}}}\left\{1 + \tfrac{1}{16}(1 + \tfrac{9}{32}(1 + \tfrac{49}{48}(1 + \dots\right\}; \tag{55}$$

which make $$u = 1{\cdot}2109, \qquad v = \frac{1{\cdot}18}{\underline{\lvert \frac{1}{4}}};$$

and therefore $$\frac{1}{\underline{\lvert \frac{1}{4}}} = 1{\cdot}024.$$

Now this shows a large error, for the value is about $1{\cdot}11$. This excess in v is, however, made a deficit by not counting the smallest term in the v series (the third term). Omitting it, we make

$$v = \frac{1{\cdot}0625}{\underline{\lvert \frac{1}{4}}} \quad \text{and} \quad \frac{1}{\underline{\lvert \frac{1}{4}}} = 1{\cdot}14.$$

Again, with $x = 2$, we have

$$u = 1 + \tfrac{1}{2} - \tfrac{3}{16}(1 - \tfrac{7}{18}(1 - \tfrac{11}{32}(1 - \tfrac{15}{50}(1 - \tfrac{19}{72}(1 - \dots,$$

$$v = \frac{2^{\frac{1}{4}}}{\underline{\lvert \frac{1}{4}}}\left\{1 + \tfrac{1}{32}(1 + \tfrac{9}{64}(1 + \tfrac{49}{96}(1 + \tfrac{121}{128}(1 + \dots\right\},$$

making $$u = 1{\cdot}365, \qquad v = \frac{1{\cdot}0399}{\underline{\lvert \frac{1}{1}}} \times 2^{\frac{1}{4}}.$$

This makes $$\frac{1}{\underline{\lvert \frac{1}{4}}} = \frac{1{\cdot}365}{1{\cdot}04 \times 1{\cdot}18} = 1{\cdot}11,$$

which is very good.

37. Now passing to the case of a bigger n, viz., $\frac{1}{2}$, we may remark that this differs from the known good case $n = -\frac{1}{2}$ by an integral differentiation, so we may expect good results again. We have

$$u = 1 + \frac{x}{2}\left(1 - \frac{x}{8}\left(1 - \frac{x}{6}\left(1 - \frac{5x}{32}\left(1 - \frac{7x}{50}\left(1 - \ldots\right.\right.\right.\right.\right., \tag{56}$$

$$v = \frac{2x^{\frac{1}{2}}}{\pi^{\frac{1}{2}}}\left(1 + \frac{1}{4x}\left(1 + \frac{1}{8x}\left(1 + \frac{9}{12x}\left(1 + \frac{25}{16x}\left(1 + \ldots\right.\right.\right.\right.\right. \tag{57}$$

Taking $x = 1$ first, giving

$$u = 1 + \tfrac{1}{2}(1 - \tfrac{1}{8}(1 - \tfrac{1}{6}(1 - \tfrac{5}{32}(1 - \ldots, \tag{58}$$

$$v = \frac{2}{\pi^{\frac{1}{2}}}\left\{1 + \tfrac{1}{4}(1 + \tfrac{1}{8}(1 + \tfrac{3}{4}(1 + \tfrac{25}{16}(1 + \ldots,\right. \tag{59}$$

we find $\qquad u = 1{\cdot}4464, \qquad v = \dfrac{2{\cdot}5625}{1{\cdot}772} = 1{\cdot}4462,$

by *not* counting the last convergent, that is, the smallest term in the v series. Its inclusion makes v appreciably too big, viz. 1·46.

Next take $x = 2$. Then

$$u = 1 + 1 - \tfrac{1}{4}(1 - \tfrac{1}{3}(1 - \tfrac{5}{16}(1 - \tfrac{7}{25}(1 - \tfrac{9}{36}(1 - \tfrac{11}{49}(1 - \ldots, \tag{60}$$

$$v = \frac{2\sqrt{2}}{\sqrt{\pi}}\left\{1 + \tfrac{1}{8}(1 + \tfrac{1}{16}(1 + \tfrac{9}{24}(1 + \tfrac{25}{32}(1 + \tfrac{49}{40}(1 + \ldots;\right. \tag{61}$$

giving $\qquad u = 1{\cdot}81275, \qquad v = \dfrac{3{\cdot}2124}{1{\cdot}772} = 1{\cdot}812,$

again not counting the smallest term.

Lastly, with $x = 3$, we have

$$u = 1 + \tfrac{3}{2} - \tfrac{9}{16}(1 - \tfrac{1}{2}(1 - \tfrac{15}{32}(1 - \tfrac{21}{50}(1 - \tfrac{27}{72}(1 - \tfrac{33}{98}(1 - \ldots, \tag{62}$$

$$v = \frac{2\sqrt{3}}{\sqrt{\pi}}\left\{1 + \tfrac{1}{12}(1 + \tfrac{1}{24}(1 + \tfrac{1}{4}(1 + \tfrac{25}{48}(1 + \tfrac{49}{60}(1 + \tfrac{81}{72}(1 + \ldots;\right. \tag{63}$$

giving $\qquad u = 2{\cdot}1260, \qquad v = 2{\cdot}1256,$

again neglecting the smallest term in v, though it is of little moment in this example. The tendency for v to be too big when the smallest term is fully counted should be noted.

38. A further increase of n to $\frac{3}{4}$ gives good results, and likewise $\frac{9}{10}$. Thus, for $\frac{9}{10}$ we have

$$u = 1 + \tfrac{9}{10}x(1 - \tfrac{1}{40}x(1 - \tfrac{11}{90}x(1 - \tfrac{21}{160}x(1 - \tfrac{31}{250}x(1 - \ldots, \tag{64}$$

$$v = \frac{x^{\frac{9}{10}}}{\left|\!\underline{\tfrac{9}{10}}\right.}\left\{1 + \frac{9^2}{100x}\left(1 + \frac{1^2}{200x}\left(1 + \frac{11^2}{300x}\left(1 + \frac{21^2}{400x}(1 + \ldots;\right.\right.\right.\right. \tag{65}$$

giving in the case of $x = 1$,

$$u = 1\cdot880, \qquad v = \frac{1\cdot815}{\underline{|\tfrac{9}{10}}}; \quad \therefore \frac{1}{\underline{|\tfrac{9}{10}}} = \frac{1\cdot880}{1\cdot815} = 1\cdot035.$$

Thus we have practically gone over the ground from $n = -\frac{1}{2}$ to $= 1$ with good results, so far as the limited examples are concerned, and there can be, so far, scarcely a doubt of the existence of numerical equivalence, in the same sense as before with respect to the ascending and descending series for the Fourier-Bessel function. It remains to examine cases between $n = -\frac{1}{2}$ and -1. This is important on account of the complete failure in the latter case of the numerical equivalence when estimated in the above manner. From the already shown indeterminateness of the binomial expansion when $n = -1$, we have the suggestion of a partial explanation, because we should arrive at the form $au + bv$, where $a + b = 1$. But there remains the fact indicated that the extreme forms of the binomial expansion are equivalent, so that we should expect u and v to be equivalent. Since, however, the numerical equivalence of the different forms of $(1 + x)^n$ becomes very unsatisfactory when n is or is near -1, so we should not be surprised to find that the unsatisfactoriness becomes emphasized in the case of u and v. Such is, in fact, the case.

Failure of Numerical Equivalence of Derived Series reckoned by Initial Convergence, at first slight, and later complete, when n approaches a Negative Integer.

39. Take $n = -\frac{3}{4}$ in (37), (38). Then

$$u = 1 - \frac{3x}{4}\left(1 - \frac{7x}{16}\left(1 - \frac{11x}{36}\left(1 - \frac{15x}{64}\left(1 - \frac{19x}{100}\right.\right.\right.\right., \tag{66}$$

$$v = \frac{1}{4x^{\frac{3}{4}}\underline{|\tfrac{1}{4}}}\left\{1 + \frac{9}{16x}\left(1 + \frac{49}{32x}\left(1 + \frac{121}{48x}\left(1 + \ldots\right.\right.\right.\right\}. \tag{67}$$

When $x = 1$, we find that

$$u = 0\cdot497, \qquad v = \frac{0\cdot25 \text{ or } 0\cdot39}{\underline{|\tfrac{1}{4}}},$$

according as we do not, or do, count the smallest term in v. That is,

$$\frac{1}{\underline{|\tfrac{1}{4}}} = \frac{0\cdot497}{0\cdot25 \text{ or } 0\cdot39}.$$

Now the first gives far too great a result, whilst the other, though not so bad, is still too great. That is, the v series gives too small a result, when the smallest term is fully included. A part of the next term is needed, to come to u.

When $x = 2$ we deduce that

$$u = 0{\cdot}28, \qquad \frac{1}{\lfloor\frac{1}{4}} = \frac{4 \times 2 \times 0{\cdot}28}{1{\cdot}28 \text{ or } 1{\cdot}49} = \frac{1{\cdot}88}{1{\cdot}28 \text{ or } 1{\cdot}49};$$

the first case being without, and the second with, the smallest term in v. Both results are too great, though the error is less than the last term counted. But this rule breaks down when we pass to $x = 3$, when we conclude that

$$u = 0{\cdot}175, \qquad \frac{1}{\lfloor\frac{1}{4}} = \frac{0{\cdot}175 \times 4 \times 3^{\frac{3}{4}}}{1{\cdot}1875 \text{ or } 1{\cdot}2813} = 1{\cdot}3437 \text{ or } 1{\cdot}2454;$$

the former case being without, and the latter with, the smallest term in v. But the result is too big, and the error rule just mentioned fails. For if we add on the smallest term a second time, we obtain 1·1604, which is still too big.

40. Since the case $n = -\frac{3}{4}$ is bad, we may expect $n = -\frac{9}{10}$ to be worse. We have

$$u = 1 - \frac{9x}{10} + \frac{9\,.\,19}{\lfloor 2 \lfloor 2}\left(\frac{x}{10}\right)^2 - \frac{9\,.\,19\,.\,29}{\lfloor 3 \lfloor 3}\left(\frac{x}{10}\right)^3 + \ldots, \tag{68}$$

$$v = \frac{1}{x^{\frac{9}{10}} \lfloor -\frac{9}{10}}\left\{1 + \frac{81}{100x} + \frac{81\,.\,361}{\lfloor 2\,(100x)^2} + \ldots\right\}. \tag{69}$$

Here take $x = 1$, then we conclude that

$$u = 0{\cdot}42, \qquad \frac{1}{\lfloor\frac{1}{10}} = \frac{4{\cdot}2}{1{\cdot}0 \text{ or } 1{\cdot}81}.$$

But it cannot lie between these limits, being only a little over unity. So add on to v the next term, the third in the v series. This will give

$$\frac{1}{\lfloor\frac{1}{10}} = \frac{4{\cdot}2}{3{\cdot}2},$$

which is still too great, and, of course, the error rule is wrong, as we suspected just now.

Whilst there does not appear to be any departure from numerical equivalence of u and v in the sense used between $n = -\frac{1}{2}$ and $n = +1$, it appears that when n is below $-\frac{1}{2}$, there is a tendency for the v series (convergent part) to give too small a result. This tendency, which is at first small, becomes pronounced when n is down to $-\frac{9}{10}$, at least for small values of x. It is likely that for large values, the rule in question might still hold good. But sinking below $-\frac{9}{10}$ towards -1 makes the tendency become a marked characteristic, and in the end the rule wholly fails except for an infinite value of x.

Success of Alternative Method of Representation by Harmonic Analysis.

41. We may then adopt another method. Thus, (44) and (45) arise from

$$u = 1-\nabla^{-1}+\nabla^{-2}-\nabla^{-3}+\dots\,, \tag{70}$$

$$v = \nabla-\nabla^{2}+\nabla^{3}-\nabla^{4}+\dots\,. \tag{71}$$

With unit operand, the u series is immediately integrable without any obscurity, giving ϵ^{-x}. The v series leads to an unintelligible result. But let the unit operand be replaced by its simple harmonic equivalent. Then

$$v = (1-\nabla+\nabla^2-\dots)\nabla = \frac{1-\nabla}{1-\nabla^2}\frac{1}{\pi}\int_0^\infty \cos mx\,dm$$

$$= (1-\nabla)\frac{1}{\pi}\int_0^\infty \frac{\cos mx}{1+m^2}\,dm$$

$$= (1-\nabla)\tfrac{1}{2}\epsilon^{-\sqrt{x^2}} = \epsilon^{-x}, \tag{72}$$

when x is positive, which is the required result. We are only concerned with positive x, but it is worth noting that when x is negative, this method makes v zero. This is also in accordance with the analytical method, or (70) directly integrated, for we suppose the operand to start when $x = 0$, and to be zero for negative x, which makes u also zero then.

42. As regards the derived formulæ (39) to (42), although I have not examined them thoroughly to ascertain limits within which the suspected numerical equivalence may obtain, I find there is a rough agreement between (41) and (42) when $n = \frac{1}{2}$ and $m = 3$, even with $x = 1$, and the convergency confined to the first three terms of v, the results being

$$u = 1+\frac{x}{2}\Big(1-\frac{x}{16}\Big(1-\frac{3x}{54}\Big(1-\frac{5x}{128}\Big(1-\dots\,, \tag{73}$$

$$v = \frac{x^{\frac{1}{3}}}{(\lfloor\frac{1}{2})^2}\Big\{1+\frac{1}{8x}\Big(1-\frac{1}{16x}\Big(1-\frac{27}{24x}\Big(1-\dots\,; \tag{74}$$

which, when $x = 1$, give

$$u = 1\cdot47, \qquad v = 1\cdot41.$$

Again, with the much larger value $x = 9$, we have

$$u = 3\cdot88, \qquad v = 3\cdot87,$$

which is a very close agreement.

This is promising as regards further numerical agreement when m

is made larger, but the promise is not fulfilled when m is as big as 10. Take $n=\frac{1}{2}$ and $m=10$ in the series (41), (42), so that

$$u = 1+\frac{x}{2}\left(1-\frac{x}{2\,.\,2^{10}}\left(1-\frac{3\,x}{2\,.\,3^{10}}\left(1-\ldots\ldots\right.\right.\right., \tag{75}$$

$$v = \frac{x^{\frac{1}{2}}}{(\lfloor\frac{1}{2})^{9}}\left\{1+\frac{1}{2^{10}x}\left(1+\frac{1}{2\,.\,2^{10}x}\left(1+\frac{3^{10}}{3\,.\,2^{10}x}\left(1+\ldots\ldots\right.\right.\right.\right. . \tag{76}$$

Here $x=1$ makes u a little less than $1\frac{1}{2}$, while the first term of v is 2·965, which is very little changed by the next two. But observe a fresh peculiarity in the v series. The change from convergency to divergency at the fourth term is so immensely rapid that this fact alone might render the series quite unsuitable for approximate numerical calculation. A portion of the term following the least term might be required (though not in the last example), but when this term is a large multiple of the least term, no definite information is obtainable.

What is the Meaning of Equivalence? Sketch of Gradual Development of Ideas concerning Equivalence and Divergent Series (up to § 49).

43. In the preceding, I have purposely avoided giving any definition of "equivalence." Believing in example rather than precept, I have preferred to let the formulæ, and the method of obtaining them, speak for themselves. Besides that, I could not give a satisfactory definition which I could feel sure would not require subsequent revision. Mathematics is an experimental science, and definitions do not come first, but later on. They make themselves, when the nature of the subject has developed itself. It would be absurd to lay down the law beforehand. Perhaps, therefore, the best thing I can do is to describe briefly several successive stages of knowledge relating to equivalent and divergent series, being approximately representative of personal experience.

(a). Complete ignorance.

(b). A convergent series has a limit, and therefore a definite value. A divergent series, on the contrary, is of infinite value, of course. So all solutions of physical problems must be in finite terms or in convergent series. Otherwise nonsense is made.

The Use of Alternating Divergent Series. Boole's Rejection of Continuous Divergent Series.

44. (c). Eye-opening. But in some physical problems divergent series are actually used for calculation. A notable example is Stokes's divergent formula for the oscillating function $J_n(x)$. He showed that the error was less than the last term included. Now

series of this kind have the terms alternately positive and negative. This seems to give a clue to the numerical meaning. The terms get bigger and bigger, but the alternation of sign prevents the assumption of an infinite value, either positive or negative. It is possible to imagine a *finite* quantity split up into parts alternately positive and negative, and of successively increasing magnitude (after a certain point, for example). It is a bad arrangement of parts, certainly, but understandable roughly by the initial convergence. So the use of alternating divergent series may be justified by numerical convenience in an approximate calculation of the value of the function.

But, by the same reasoning, a direct divergent series, with all terms of one sign, is of infinite value, and therefore out of court. It cannot have a finite value, and cannot be the solution of a physical problem involving finite values. This seems to be what Boole meant in his remark on p. 475 of his 'Differential Equations' (3rd edition) :—"It is known that in the employment of divergent series an important distinction exists between the cases in which the terms of the series are ultimately all positive, and alternately positive and negative. In the latter case we are, according to a known law, permitted to employ that portion of the series which is convergent for the calculation of the entire value." He proceeded to exemplify this by Petzval's integrals. The argument is equivalent to this. Change the sign of x in the Series C, equation (3) above. Let the result be C′. Then we must use the Series C′ when x is positive, and C when x is negative. This amounts to excluding the direct divergent series altogether, and using only the alternating. That is, we have one solution, not two. Professor Boole did not say what the "known law" was. His above authoritative rejection of direct divergent series led me away from the truth for many years. The plausibility of the argument is evident, as evident as that the value of a direct divergent series is infinity.

Divergent Series as Differentiating Operators.

45. (*d*). Later on, divergent series presented themselves in an entirely different manner. In the solution of physical problems by means of differentiating or analytical operators, the operators themselves may be either convergent, or alternatingly divergent, or directly divergent. That is, they are so when regarded algebraically, with a differentiator regarded as a quantity. When the operations indicated by the operator are carried out upon a function of the variable, the solution of the problem arises, and in a convergent form. Here, then, we have the secret of the direct divergent series at last. It is numerically meaningless, when considered algebraically, with a quantity and its powers involved. But analytically considered, the question of divergency does not arise. The proper use of divergent series is as

analytical operators to obtain convergent algebraical solutions. The series C and C′ above referred to are then truly the two independent solutions of a certain differential equation, and neither should be rejected, for they are natural companions.

Disappearance of the Distinction between Direct and Alternating Divergent Series.

46. (*e*). But, still pursuing the subject along the same lines, this view is soon found to be imperfect. For a given operator leading to a convergent solution one way may lead to a divergent solution by another. Or it may lead to the same algebraical function by diverse ways. These and other considerations show that divergent series, even when continuously divergent, must be considered numerically as well as algebraically and analytically. But in the analytical use of a direct or continuously divergent series every term must be used, if the result is a convergent series. Yet it is plain that we cannot count the whole divergent series numerically, because it has no limit. And on examination we find that the initial convergent part of the continuously divergent series gives the value of the function in the same sense as an alternatingly divergent series. In the latter case we come nearest to the value by stopping at the smallest term, where the oscillation is least. If we now make all terms positive, so that the series is continuously divergent, and treat it in the same way, and stop when the addition made by a fresh term is the smallest, we come near the true value.

We now seem to have something like a distinct theory of divergent series. The supposed distinction between the alternating and the continuous divergent series has disappeared. Analytical equivalence of two series, one convergent, the other divergent, may require all terms in the divergent one to be counted. Numerical equivalence exists also, but is governed by the initial convergency.

Broader and Deeper Views obtained by the Generalized Calculus. Analytical, Numerical, and Algebraical Equivalences. Equivalence not necessarily Identity.

47. (*f*). The last view is a distinct advance, and it is certainly true in the case of many equivalences, including some which are of importance in mathematical physics. But, again, further examination shows that the last word has not been said. For on seeking to explain the meaning and origin of equivalent series, we are led to a theory of generalized differentiation, involving the inverse factorial as a completely continuous function both ways, and to methods of multiplying equivalent forms to any extent, and in a generalized manner, all previous examples being merely special extreme cases of

the general results. We also come to confirm the idea we have recognized that equivalence may be understood in three distinct senses. viz., analytical, algebraical, and numerical. The first use made by me of equivalent series, one of which is continuously divergent, was analytical only. The second use was numerical. The third is algebraical, through the generalized algebraical theorems. We also see that equivalence does not necessarily or usually mean identity, Thus the series A, B, C are analytically, algebraically, and numerically equivalent with x positive. But they are not algebraically identical. The identity is given by $C = \frac{1}{2}(A+B)$. This point is rather important in some transformations, and explains some previously inexplicable peculiarities. Thus, the series A is real whether x be real or a pure imaginary. In the latter case, we get the oscillating function $J_0(x)$, the original Fourier cylinder function. But the equivalent series C becomes complex by the same transformation. The above-mentioned identity explains it. The second solution of the oscillating kind is brought in, as will appear a little later (§ 70).

Partial Failure of Interpretation of Numerical Value of Divergent Series by Initial Convergence. Further Explanation yet required.

48. (g). But whilst we thus greatly extend our views concerning divergent series, the question of numerical equivalence, which just now in (f) seemed to be about settled, becomes again obscured. The property that the value of a divergent series, including the continuously divergent, may be estimated by the initially convergent part, is a very valuable one. But the property is not generally true, and, in fact, sometimes fails in a very marked manner. We must, therefore, reserve for the present the question of numerical equivalence in general, and let the explanation evolve itself in course of time. If definitely understandable numerical equivalence of series were imperative under all circumstances, then I am afraid that the study of the subject would be of doubtful value. But the matter has not this limited range, a very important application of divergent series being their analytical use, which is free from the numerical difficulty. For example, the extreme forms of the binomial theorem may, when considered numerically equivalent, be utterly useless. Yet they may be employed to lead to other series, either convergent, or it may be divergent, but with a satisfactory initial convergence contrasting with the original. Note that the series may sometimes take the form of definite integrals, apparently of infinite or of indefinite value. In any case we should not be misled by apparent unintelligibility to ignore the subject. That is not the way to get on. We have seen the error fallen into by Boole and others on the subject of divergent series. It is not so long ago, either, since mathematicians

of the highest repute could not see the validity of investigations based upon the use of the algebraic imaginary. The results reached were, according to them, to be regarded as suggestive merely, and required proof by methods not involving the imaginary. But familiarity has bred contempt, and at the present day the imaginary is a generally used powerful engine, which I should think most mathematicians consider can be trusted (if well treated) to give valid proofs, though it certainly does need cautious treatment sometimes, and perhaps auxiliary aid.*

Application of Generalized Binomial Theorem to obtain a Generalized Formula for $\log x$.

49. Let us now pass on to view the logarithm in its generalized aspect. One way of generalizing $\log x$ is to regard it as the limit of $(d/dn)x^n$ when $n = 0$. Now, using the generalized binomial theorem

$$(1 + x)^n = \Sigma \frac{x^r \underline{|n}}{\underline{|r}\,\underline{|n-r}}, \tag{77}$$

where r has any value and the step is unity, we obtain by this process

$$\log(1+x) = \Sigma \frac{x^r}{\underline{|r}} \frac{d}{dn}\left(\frac{\underline{|n}}{\underline{|n-r}}\right)_{(n=0)}$$

$$= \Sigma \frac{x^r}{\underline{|r}\,\underline{|-r}}\left(\frac{(\underline{|0})'}{\underline{|0}} - \frac{(\underline{|-r})'}{\underline{|-r}}\right) \tag{78}$$

where the accent means differentiation to n, after which the special values are given to the argument. Or, since

$$\frac{1}{\underline{|r}}\frac{1}{\underline{|-r}} = \frac{\sin r\pi}{r\pi}, \qquad \text{and} \qquad \frac{(\underline{|n})'}{\underline{|n}} = -\frac{f'(n)}{f(n)}, \tag{79}$$

if $f(n)$ is the inverse factorial, therefore

$$\log(1+x) = \Sigma\, x^r \frac{\sin r\pi}{r\pi}\left(\frac{f'(-r)}{f(-r)} - \frac{f'(0)}{f(0)}\right). \tag{80}$$

But also

$$f(0) = 1, \qquad f'(0) = \mathrm{C} = 0{\cdot}5772, \qquad \Sigma\, x^r \frac{\sin r\pi}{r\pi} = 1,$$

by § 17 and equation (94), Part I. So we reduce to

$$\log(1+x) = -\mathrm{C} + \Sigma\, x^r f(r) f'(-r). \tag{81}$$

* Perhaps we may fairly regard the theory of generalized analysis as being now in the same stage of development as the theory of the imaginary was before the development of the modern theory of functions. Not that I know much about the latter; the big book lately turned out by Forsyth reveals to me quite unexpected developments.

50. To obtain the common formula for the logarithm, take $r=0$. Then, since

$$f'(-1)=1,\quad f'(-2)=-1,\quad f'(-3)=|\underline{2},\quad f'(-4)=-|\underline{3},\quad \&c.,$$

we reduce (81) to

$$\log(1+x) = -\mathrm{C}+f'(0)+xf'(-1)+\frac{x^2}{|\underline{2}}f'(-2)+\frac{x^3}{|\underline{3}}f'(-3)+\dots$$

$$= x-\tfrac{1}{2}x^2+\tfrac{1}{3}x^3-\tfrac{1}{4}x^4+\dots\,. \tag{82}$$

When $r=\frac{1}{2}$ in (81), we have

$$\log(1+x) = -\mathrm{C}+\frac{2}{\pi}\left\{x^{\frac{1}{2}}\frac{f'(-\frac{1}{2})}{f(-\frac{1}{2})}-\tfrac{1}{3}x^{\frac{3}{2}}\frac{f'(-\frac{3}{2})}{f(-\frac{3}{2})}+\tfrac{1}{5}x^{\frac{5}{2}}\frac{f'(-\frac{5}{2})}{f(-\frac{5}{2})}-\dots\right.$$

$$\left.+x^{-\frac{1}{2}}\frac{f'(\frac{1}{2})}{f(\frac{1}{2})}-\tfrac{1}{3}x^{-\frac{3}{2}}\frac{f'(\frac{3}{2})}{f(\frac{3}{2})}+\tfrac{1}{5}x^{-\frac{5}{2}}\frac{f'(\frac{5}{2})}{f(\frac{5}{2})}-\dots\right\}. \tag{83}$$

Now here all the differential coefficients of the inverse factorials may be put in terms of $f'(-\frac{1}{2})$ by means of the formula

$$f(n)+nf'(n) = f'(n-1), \tag{84}$$

which follows from

$$nf(n) = f(n-1); \tag{85}$$

but since the resulting formula does not seem to be useful, and is complicated, it need not be given here.

Deduction of Formula for $(1+x)^{-1}$.

51. If we differentiate (81) with respect to x, we obtain

$$\frac{1}{1+x} = \Sigma\, x^{r-1}f(r-1)f'(-r)$$

$$= \Sigma\, x^r f(r)\, f'(-r-1), \tag{86}$$

where the second form of the series is got by increasing r by unity in the first. Here note that we have a definite expansion, whereas in § 32 we found the binomial expansion to be indeterminate. When $r=0$ in (86) we have, of course, the special form $1-x+x^2-\dots$. It is also right when $r=\frac{1}{2}$.

Deduction of Formula for ϵ^{-x}.

52. Now regard (86) as true analytically, and we can obtain a formula for ϵ^{-x}. For, first put ∇^{-1} for x, giving

$$\frac{1}{1+\nabla^{-1}} = \Sigma\, \nabla^{-r} f(r)\, f'(-r-1). \tag{87}$$

Integrating, we obtain

$$\epsilon^{-x} = \Sigma\, x^r [f(r)]^2 f'(-r-1). \tag{88}$$

This is quite correct when $r=0$, when we obtain the ordinary formula $1-x+\ldots$. Another form of (88) is

$$\epsilon^{-x} = -\Sigma\, x^r (\underline{|-r-1})' \frac{\sin^2(r+1)\pi}{\pi^2}. \tag{89}$$

Now when $r=\frac{1}{2}$, the square of the sine equals unity throughout, giving

$$\epsilon^{-x} = -\frac{1}{\pi^2}\Big\{ (\underline{|-\tfrac{1}{2}})' x^{-\frac{1}{2}} + (\underline{|-\tfrac{3}{2}})' x^{\frac{1}{2}} + (\underline{|-\tfrac{5}{2}})' x^{\frac{3}{2}} + (\underline{|-\tfrac{7}{2}})' x^{\frac{5}{2}} + \ldots$$
$$+ (\underline{|\tfrac{1}{2}})' x^{-\frac{3}{2}} + (\underline{|\tfrac{3}{2}})' x^{-\frac{5}{2}} + (\underline{|\tfrac{5}{2}})' x^{-\frac{7}{2}} + \ldots \Big\}. \tag{90}$$

Since we also have

$$\epsilon^{x} = \frac{x^{\frac{1}{2}}}{\underline{|\tfrac{1}{2}}} + \frac{x^{\frac{3}{2}}}{\underline{|\tfrac{3}{2}}} + \ldots + \frac{x^{-\frac{1}{2}}}{\underline{|-\tfrac{1}{2}}} + \ldots, \tag{91}$$

the product of (90) and (91) should be unity. That is,

$$-\pi^2 = \Big\{ \frac{(\underline{|-\frac{1}{2}})'}{\underline{|\frac{1}{2}}} + \frac{(\underline{|-\frac{3}{2}})'}{\underline{|-\frac{1}{2}}} + \frac{(\underline{|-2\frac{1}{2}})'}{\underline{|-1\frac{1}{2}}} + \frac{(\underline{|-3\frac{1}{2}})'}{\underline{|-2\frac{1}{2}}} + \ldots$$
$$+ \frac{(\underline{|\frac{1}{2}})'}{\underline{|1\frac{1}{2}}} + \frac{(\underline{|1\frac{1}{2}})'}{\underline{|2\frac{1}{2}}} + \frac{(\underline{|2\frac{1}{2}})'}{\underline{|3\frac{1}{2}}} + \ldots \Big\}$$
$$+ x\Big\{ \frac{(\underline{|-\frac{1}{2}})'}{\underline{|1\frac{1}{2}}} + \frac{(\underline{|-1\frac{1}{2}})'}{\underline{|\frac{1}{2}}} + \frac{(\underline{|-2\frac{1}{2}})'}{\underline{|-\frac{1}{2}}} + \ldots + \frac{(\underline{|\frac{1}{2}})'}{\underline{|2\frac{1}{2}}} + \frac{(\underline{|1\frac{1}{2}})'}{\underline{|3\frac{1}{2}}} \ldots \Big\}$$
$$+ x^2\Big\{ \frac{(\underline{|-\frac{1}{2}})'}{\underline{|2\frac{1}{2}}} + \frac{(\underline{|-1\frac{1}{2}})'}{\underline{|1\frac{1}{2}}} + \frac{(\underline{|-2\frac{1}{2}})'}{\underline{|\frac{1}{2}}} + \ldots + \frac{(\underline{|\frac{1}{2}})'}{\underline{|3\frac{1}{2}}} + \frac{(\underline{|1\frac{1}{2}})'}{\underline{|4\frac{1}{2}}} + \ldots \Big\}$$
$$+ \ldots \tag{92}$$

Going by the ordinary principles of the algebra of convergent series, we should conclude that the coefficient of x^0 was $-\pi^2$, and that the coefficients of the other powers of x were zero. But this rule is not generally true in series of the present kind, as we have already exemplified. Therefore, to see how it goes in the immediate case, I have calculated the value of the coefficient of x^0. By (84) we have

$$\frac{1}{n}+\frac{f'(n)}{f(n)}=\frac{f'(n-1)}{f(n-1)}, \tag{93}$$

and from this we may derive, when r is a positive integer,

$$-\frac{f'(r+\frac{1}{2})}{f(r+\frac{1}{2})}=2+\tfrac{2}{3}+\tfrac{2}{5}+\tfrac{2}{7}+\ldots.+\frac{1}{r+\frac{1}{2}}-\frac{f'(-\frac{1}{2})}{f(-\frac{1}{2})}, \tag{94}$$

and also

$$-\frac{f'(-r-1\frac{1}{2})}{f(-r-1\frac{1}{2})}=\frac{1}{r+\frac{1}{2}}+\frac{1}{r-\frac{1}{2}}+\frac{1}{r-\frac{3}{2}}+\ldots.+\frac{1}{1\frac{1}{2}}+2-\frac{f'(-\frac{1}{2})}{f(-\frac{1}{2})}. \tag{95}$$

Therefore, when r is positively integral, we have

$$\frac{f'(r+\frac{1}{2})}{f(r+\frac{1}{2})}=\frac{f'(-r-1\frac{1}{2})}{f(-r-1\frac{1}{2})}, \tag{96}$$

which makes the coefficient of x^0 in (92) become

$$-2\mathrm{F}+(\tfrac{2}{3}-2)(2-\mathrm{F})+(\tfrac{2}{5}-\tfrac{2}{3})(\tfrac{2}{3}+2-\mathrm{F})+(\tfrac{2}{7}-\tfrac{2}{5})(\tfrac{2}{5}+\tfrac{2}{3}+2-\mathrm{F})+\ldots.,$$

where, for brevity, F stands for $f'(-\frac{1}{2})/f(-\frac{1}{2})$. It is readily seen that the complete coefficient of F vanishes, and the remainder reduces to

$$-\left(4+\frac{4}{3^2}+\frac{4}{5^2}+\frac{4}{7^2}+\ldots.\right)=-\frac{\pi^2}{2}. \tag{97}$$

Therefore the coefficient of x^0 in (92) contributes one-half of the total, and the other half must be given by (or rather, be equivalent to) the sum of the terms involving x. Although I have not thoroughly investigated this, there did not appear to be any inconsistency.

Remarks on Equivalences in Factorial Formulæ. Verifications.

53. If it is given that

$$\mathrm{F}(x)=\Sigma\, x^r\phi(r), \tag{98}$$

it does not, as already remarked in effect, follow that $\phi(r)$ is a definitely unique function of r. But it is sometimes true, and then the equation

$$0=\Sigma\, x^r\phi(r) \tag{99}$$

may require the vanishing of every coefficient. For example, using (88) above, if we differentiate it to x we obtain

$$\begin{aligned}-\epsilon^{-x}&=\Sigma\, x^{r-1}f(r-1)f(r)f'(-r-1)\\ &=\Sigma\, x^r[f(r)]^2\frac{f'(-r-2)}{r+1}.\end{aligned} \tag{100}$$

Therefore, by adding this equation to (88), we obtain

$$0 = \Sigma\, x^r [f(r)]^2 \left\{ f'(-r-1) + \frac{f'(-r-2)}{r+1} \right\}. \tag{101}$$

Now it is a fact that this is true, term by term, when $r = 0, 1, 2, 3,$ &c. But (101) is not true in the same manner generally. Only when $f(n) = 0$, that is, when n is a negative integer, do we have

$$nf'(n) = f'(n-1), \tag{102}$$

by (93), which is general. Put $n = -r-1$ to suit (101). But

$$f'(-r-1) - \frac{f'(-r-2)}{-r-1} = \frac{f(-r-1)}{r+1}, \tag{103}$$

by (93). Therefore (101) is the same as

$$0 = \Sigma\, x^r [f(r)]^2 \frac{f(-r-1)}{r+1} = \Sigma \frac{x^r}{\underline{|r}} \frac{\sin(r+1)\pi}{(r+1)\pi}, \tag{104}$$

which does not vanish term by term, except for the special values of r indicated. Integrating (104), we obtain

$$\text{constant} = \Sigma \frac{x^r}{\underline{|r}} \frac{\sin r\pi}{r\pi}. \tag{105}$$

54. The case $r = 0$ we have already had, when the constant is 1, so it should be 1 generally. The case $r = \frac{1}{2}$ is represented by

$$1 = \frac{2}{\pi} \left\{ \frac{x^{\frac{1}{2}}}{\underline{|\tfrac{1}{2}}} + \frac{x^{-\frac{1}{2}}}{\underline{|-\tfrac{1}{2}}} - \tfrac{1}{3}\left(\frac{x^{\frac{3}{2}}}{\underline{|\tfrac{3}{2}}} + \frac{x^{-\frac{3}{2}}}{\underline{|-\tfrac{3}{2}}} \right) + \tfrac{1}{5}\left(\frac{x^{\frac{5}{2}}}{\underline{|\tfrac{5}{2}}} + \frac{x^{-\frac{5}{2}}}{\underline{|-\tfrac{5}{2}}} \right) - \dots \right\}, \tag{106}$$

and the following is a verification :—The right member is

$$\frac{2}{\pi}(\nabla^{-\frac{1}{2}} - \tfrac{1}{3}\nabla^{-\frac{3}{2}} + \tfrac{1}{5}\nabla^{-\frac{5}{2}} - \dots + \nabla^{\frac{1}{2}} - \tfrac{1}{3}\nabla^{\frac{3}{2}} + \tfrac{1}{5}\nabla^{\frac{5}{2}} - \dots)$$

$$= \frac{2}{\pi}(\tan^{-1}\nabla^{-\frac{1}{2}} + \tan^{-1}\nabla^{\frac{1}{2}}) = \frac{2}{\pi}\tan^{-1}\frac{\nabla^{-\frac{1}{2}} + \nabla^{\frac{1}{2}}}{1 - \nabla^{\frac{1}{2}}\nabla^{-\frac{1}{2}}}$$

$$= \frac{2}{\pi}\tan^{-1}\infty = 1. \tag{107}$$

Although the validity of this process of evaluation may be doubted, there is no inconsistency exhibited.

55. The other formula of a similar kind, viz.,

$$1 = \Sigma \frac{x^r}{\underline{|r}\,\underline{|-r}} = \Sigma\, x^r \frac{\sin r\pi}{r\pi}, \tag{108}$$

when similarly treated, gives

$$1 = \Sigma \, \nabla^{-r} f(-r) = \Sigma \, \nabla^r f(r) = \epsilon^{\nabla}. \tag{109}$$

That is, $\epsilon^{\nabla} 1 = 1$, which is a case of Taylor's theorem, if we do not go too close to the boundary where the operand begins. That is, regarding the operand as $\mathrm{F}(x)$, it is turned to $\mathrm{F}(x+1)$.

Application of Generalized Exponential to obtain other Generalized Formulæ involving the Logarithm.

56. Now return to the fundamental exponential formula

$$\epsilon^x = \Sigma \, x^r f(r), \tag{110}$$

and derive from it some other logarithmic formulæ. Differentiate to r, then

$$0 = \epsilon^x \log x + \Sigma \, x^r f'(r). \tag{111}$$

A second differentiation to r gives

$$0 = -\epsilon^x (\log x)^2 + \Sigma \, x^r f''(r). \tag{112}$$

A third differentiation gives

$$0 = \epsilon^x (\log x)^3 + \Sigma \, x^r f'''(r), \tag{113}$$

and so on. Or, all together,

$$\log x = -\frac{\Sigma \, x^r f'(r)}{\Sigma \, x^r f(r)} = -\frac{\Sigma \, x^r f''(r)}{\Sigma \, x^r f'(r)} = -\frac{\Sigma \, x^r f'''(r)}{\Sigma \, x^r f''(r)} = \dots\dots \tag{114}$$

Now combine them to see if they fit. Thus, we have the elementary formula

$$x\epsilon^x = \epsilon^x \left\{ 1 + \log x + \frac{(\log x)^2}{\underline{|2}} + \dots\dots \right\}, \tag{115}$$

and this, by the use of (114), becomes

$$\Sigma \, x^r \left(f - f' + \frac{f''}{\underline{|2}} - \frac{f'''}{\underline{|3}} + \dots\dots \right)(r), \tag{116}$$

which, by Taylor's theorem, is the same as

$$\Sigma \, x^r f(r-1) = x \, \Sigma \, x^r f(r) = x\epsilon^x, \tag{117}$$

as required.

57. Again, differentiate (111) to x. We obtain

$$\begin{aligned} 0 &= \epsilon^x \log x + \epsilon^x x^{-1} + \Sigma \, r x^{r-1} f'(r) \\ &= -\Sigma \, x^r f'(r) + \epsilon^x x^{-1} + \Sigma \, x^r (r+1) f'(r+1), \end{aligned} \tag{118}$$

by using (111) again, and (110). So

$$\epsilon^x = \Sigma\, x^{r+1} f'(r) - \Sigma\, x^{r+1}(r+1) f'(r+1)$$

$$= \Sigma\, x^r\{f'(r-1) - rf'(r)\}. \tag{119}$$

Here the factor of x^r is identical with $f(r)$, by (84), which corroborates.

58. Returning to (111), if we try to make a series for $\log x$ in powers of x we obtain

$$-\log x = \Sigma\, x^r\left(1 - x + \frac{x^2}{\underline{|2}} - \dots\right) f'(r)$$

$$= \Sigma\, x^r\left\{f'(r) - f'(r-1) + \frac{1}{\underline{|2}} f'(r-2) - \frac{1}{\underline{|3}} f'(r-3) + \dots\right\}. \tag{120}$$

This is done by making x^r be the representative power throughout, by reducing the value of r by unity in the second term in the first series, by two in the third term, and so on. Or

$$= \Sigma\, x^r \frac{d}{dr}\left\{f(r) - f(r-1) + \frac{1}{\underline{|2}} f(r-2) - \frac{1}{\underline{|3}} f(r-3) + \dots\right\} \tag{121}$$

$$= \Sigma\, x^r \frac{d}{dr} \frac{1 - r + \dfrac{r(r-1)}{\underline{|2}} - \dots}{\underline{|r}} = \Sigma\, x^r \frac{d}{dr} \frac{(1-1)^r}{\underline{|r}}. \tag{122}$$

This is striking, but not usable.

Also, if we try to get a series for x^{-1} we fail. The property (84) comes in, and brings us to $x^{-1} = x^{-1}$ in the end. This failure is not obvious *a priori* in factorial mathematics.

Deduction of a Special Logarithmic Formula.

59. Now let the formula (111) be specialized by taking $r = 0$. We then have

$$-\log x = \epsilon^{-x}[f'(0) + xf'(1) + x^2 f'(2) + \dots$$

$$+ x^{-1} f'(-1) + x^{-2} f'(-2) + \dots]. \tag{123}$$

Here, for the negative values of n we have

$$f'(-1) = 1, \quad f'(-2) = -1, \quad f'(-3) = \underline{|2}, \quad f'(-4) = -\underline{|3}, \tag{124}$$

and so on, whilst for the positive we have

$$f'(0) = C, \quad f'(1) = C - 1, \quad f'(2) = \tfrac{1}{2}(C - 1 - \tfrac{1}{2}),$$

$$f'(n) = \frac{1}{\underline{|n}}\left\{C - \left(1 + \tfrac{1}{2} + \tfrac{1}{3} + \dots + \frac{1}{n}\right)\right\}, \tag{125}$$

by (84). Using these in (123) we obtain

$$-(\log x+C) = \epsilon^{-x}\left(\frac{\underline{|0}}{x}-\frac{\underline{|1}}{x^2}+\frac{\underline{|2}}{x^3}-\frac{\underline{|3}}{x^4}+\dots\right)$$

$$-\epsilon^{-x}\left\{x+\frac{x^2}{\underline{|2}}(1+\tfrac{1}{2})+\frac{x^3}{\underline{|3}}(1+\tfrac{1}{2}+\tfrac{1}{3})+\dots\right\}. \qquad (126)$$

The first series is the ordinary expression for $\epsilon^{-x}x^{-1}$ with the terms inverted, whilst the latter contains a reminiscence of the companion to the Fourier cylinder function.

60. To see whether there is a notable convergency for calculation, take $x = 2$. Then

$$\epsilon^{-2} = 0{\cdot}1353,$$

$$1-\frac{1}{x}+\frac{\underline{|2}}{x^2}-\dots = 1-\tfrac{1}{2}+\tfrac{1}{2}-\tfrac{6}{8}+\dots .$$

This is evidently about $\frac{3}{4}$ by the look of it, especially when diagrammatically represented. Also

$$x+\frac{x^2}{\underline{|2}}(1+\tfrac{1}{2})+\dots = 9{\cdot}7479.$$

So (126) gives

$$\begin{aligned}\log 2 &= 0{\cdot}1353\times 9{\cdot}7479-0{\cdot}5772-0{\cdot}375\times 0{\cdot}1353\\ &= 0{\cdot}6909.\end{aligned}$$

By common logarithmic tables we find $\log 2 = 0{\cdot}6923$. The difference is $0{\cdot}0014$. Doing it another way, we may prove by multiplication that

$$\epsilon^{-x}\left\{x+\frac{x^2}{\underline{|2}}(1+\tfrac{1}{2})+\frac{x^3}{\underline{|3}}(1+\tfrac{1}{2}+\tfrac{1}{3})+\dots\right\}$$

$$= x-\tfrac{1}{2}\frac{x^2}{\underline{|2}}+\tfrac{1}{3}\frac{x^3}{\underline{|3}}-\tfrac{1}{4}\frac{x^4}{\underline{|4}}+\dots, \qquad (127)$$

which is an interesting transformation. This, with $x = 2$, gives $1{\cdot}3203$, and produces a much closer agreement. It is probably fortuitous.

Independent Establishment of the Last.

61. We can establish (126) independently thus:—We have

$$\frac{1}{x} = \frac{\epsilon^{-x}}{x}+\frac{1-\epsilon^{-x}}{x} \qquad (128)$$

$$= \frac{\epsilon^{-x}}{x}+1-\frac{x}{\underline{|2}}+\frac{x^2}{\underline{|3}}-\frac{x^3}{\underline{|4}}+\dots . \qquad (129)$$

Integrate to x. Then

$$\log x + C = -\epsilon^{-x}\left(\frac{1}{x} - \frac{\lfloor 1}{x^2} + \frac{\lfloor 2}{x^3} - \dots\right) + x - \tfrac{1}{2}\frac{x^2}{\lfloor 2} + \tfrac{1}{3}\frac{x^3}{\lfloor 3} - \dots, \quad (130)$$

when C is some constant introduced by the integration. To find it, note that the series with the exponential factor vanishes when x is infinite; so (130) gives

$$C = x - \tfrac{1}{2}\frac{x^2}{\lfloor 2} + \tfrac{1}{3}\frac{x^3}{\lfloor 3} - \dots - \log x, \quad \text{with } x = \infty. \quad (131)$$

It is not immediately obvious that the function preceding the logarithm in (131) increases infinitely with x. But by (127) we may regard it as the ratio

$$\frac{x + \frac{x^2}{\lfloor 2}(1+\frac{1}{2}) + \frac{x^3}{\lfloor 3}(1+\frac{1}{2}+\frac{1}{3}) + \dots}{1 + x + \frac{x^2}{\lfloor 2} + \frac{x^3}{\lfloor 3} + \dots}, \quad (132)$$

and we see that the terms in the numerator become infinitely greater than those to correspond in the denominator.

A Formula for Euler's Constant.

62. Next examine whether (131) gives a rapid approximation to the value of C. When $x = 1$ we get

$$1 - \tfrac{1}{4} + \tfrac{1}{18} - \tfrac{1}{96} + \dots - 0 = 0{\cdot}77, \text{ say.}$$

When $x = 2$ we get $\quad 1{\cdot}3203 - 0{\cdot}6903 = 0{\cdot}6300.$

When $x = 3$ we get $\quad 1{\cdot}6888 - 1{\cdot}1098 = 0{\cdot}5790.$

So with $x = 3$ the error is about $\frac{1}{500}$ only. The usual formula

$$C = 1 + \tfrac{1}{2} + \tfrac{1}{3} + \dots + \frac{1}{r} - \log r, \qquad \text{with } r = \infty,$$

is very slow. Ten terms make 0·62. Twenty make about 0·602, which is still far wrong. We see that (131) will give C pretty quickly with a moderate value of x.

63. In passing, we may note that the function

$$x + \frac{x^2}{\lfloor 2}(1+\tfrac{1}{2}) + \frac{x^3}{\lfloor 3}(1+\tfrac{1}{2}+\tfrac{1}{3}) + \dots \quad (133)$$

is represented by

$$\nabla^{-1} + \nabla^{-2}(1+\tfrac{1}{2}) + \nabla^{-3}(1+\tfrac{1}{2}+\tfrac{1}{3}) + \dots, \quad (134)$$

and also by

$$\epsilon^x\left(x-\tfrac{1}{2}\frac{x^2}{\underline{|2}}+\tfrac{1}{3}\frac{x^3}{\underline{|3}}-\ldots\right)=\epsilon^x(\nabla^{-1}-\tfrac{1}{2}\nabla^{-2}+\tfrac{1}{3}\nabla^{-3}-\ldots) \tag{135}$$

$$=\epsilon^x\log(1+\nabla^{-1})=\log\left(\frac{\nabla}{\nabla-1}\right)\epsilon^x=\frac{\nabla}{\nabla-1}\log\frac{\nabla}{\nabla-1}, \tag{136}$$

which may be useful later.

Deduction of Second Kind of Bessel Function, $K_0(x)$, from the Generalized Formula of the First Kind.

64. A similar treatment of the generalized formula for the Fourier-Bessel function leads to the companion function. Thus, take

$$I_0(x)=\Sigma\, y^r[f(r)]^2, \tag{137}$$

as in (76) Part I, the value of y being $\frac{1}{4}x^2$. Differentiate to r. Then

$$0=I_0(x)\log y+2\Sigma\, y^r f(r)f'(r). \tag{138}$$

Here take the special case $r=0$. Then we have

$$0=I_0(x)\log y$$
$$+2\left\{C+y(C-1)+\frac{y^2}{(\underline{|2})^2}(C-1-\tfrac{1}{2})+\frac{y^3}{(\underline{|3})^2}(C-1-\tfrac{1}{2}-\tfrac{1}{3})+\ldots\right\}$$
$$+2\left\{\frac{y^{-1}}{\underline{|-1}}-\frac{y^{-2}}{\underline{|-2}}\underline{|1}+\frac{y^{-3}}{\underline{|-3}}\underline{|2}-\ldots\right\}. \tag{139}$$

The third line is apparently zero. But it must, as we shall see, be retained, though in a changed form. Or

$$\frac{y^{-1}}{\underline{|-1}}-\frac{y^{-2}}{\underline{|-2}}\underline{|1}+\frac{y^{-3}\,\underline{|2}}{\underline{|-3}}-\ldots=y+\frac{y^2}{(\underline{|2})^2}(1+\tfrac{1}{2})+\frac{y^3}{(\underline{|3})^2}(1+\tfrac{1}{2}+\tfrac{1}{3})+\ldots$$
$$-I_0(x)(\log y^{\frac{1}{2}}+C). \tag{140}$$

Another way. Automatic Standardization.

65. Now the right member is certainly not zero, for it represents the companion of $I_0(x)$, as may be proved in various ways, classical and unclassical. One way is from the formula for $I_n(x)$, thus,

$$I_n(x)=\frac{y^{\frac{n}{2}}}{\underline{|n}}\left(1+\frac{y}{\underline{|1}\,(1+n)}+\frac{y^2}{\underline{|2}\,(1+n)(2+n)}+\ldots\right). \tag{141}$$

When n is not an integer, $\mathrm{I}_n(x)$ and $\mathrm{I}_{-n}(x)$ are different, and represent two independent solutions of the characteristic differential equation. But when n is any integer, positive or negative, they become identical, so only one solution is got. Then another is (when $n=0$) represented by the rate of variation of $\mathrm{I}_n(x)$ with n when $n=0$. Thus,

$$-\frac{d\mathrm{I}_n(x)}{dn} = -\mathrm{I}_n(x)\left\{\tfrac{1}{2}\log y + \frac{f'(n)}{f(n)}\right\}$$
$$+\frac{y^{\frac{n}{2}}}{\underline{|n}}\left\{\frac{y}{\underline{|1}\,(1+n)^2} + \frac{y^2\left(\dfrac{1}{1+n}+\dfrac{1}{2+n}\right)}{\underline{|2}\,(1+n)(2+n)} + \dots\right\}, \tag{142}$$

which, when $n=0$, is by inspection the function on the right side of (140). Notice that this method of obtaining the second solution, like the just preceding method, gives it immediately in the form properly standardized so as to vanish at infinity. The constant C comes in automatically, and requires no separate evaluation.

The Operator producing $\mathrm{K}_0(x)$.

66. But our immediate object of attention should be the function on the left side of (140). How it can be equivalent to the right member is a mystery. It is certainly an extreme form, if correct. We may write it in the form

$$\Delta - \Delta^2\underline{|1} + \Delta^3\underline{|2} - \Delta^4\underline{|3} + \dots, \tag{143}$$

where Δ is d/dy. Now the other function $\mathrm{I}_0(x)$ is

$$1 + \frac{\Delta^{-1}}{\underline{|1}} + \frac{\Delta^{-2}}{\underline{|2}} + \frac{\Delta^{-3}}{\underline{|3}} + \dots, \tag{144}$$

without any mystery, and we see at once that these forms are analogous to

$$\epsilon^{-x} = \Delta - \Delta^2 + \Delta^3 - \Delta^4 + \dots, \tag{145}$$

$$\epsilon^{x} = 1 + \Delta^{-1} + \Delta^{-2} + \Delta^{-3} + \dots, \tag{146}$$

the latter, corresponding to (144), being obvious, whilst the former, analogous to (143), is an extreme form already considered and explained; see equations (71), (72). The unintelligibility of (143) is no evidence of its inaccuracy. More puzzling things than it have been cleared up.

67. We may also employ the special formula (126), of which we had separate verifications. Multiply it by ϵ^x and then write Δ^{-1} for x. Thus,

$$0 = (C + \log \Delta^{-1})\epsilon^{\Delta^{-1}} + (\Delta - \Delta^2 \underline{|1} + \Delta^3 \underline{|2} - \Delta^4 \underline{|3} + \ldots)$$

$$-\left(\Delta^{-1} + \frac{\Delta^{-2}}{\underline{|2}}(1+\tfrac{1}{2}) + \ldots\right), \qquad (147)$$

where we see that the operator (143) appears. Integrating, we have

$$\Delta - \Delta^2 \underline{|1} + \ldots = y + \frac{y^2}{(\underline{|2})^2}(1+\tfrac{1}{2}) + \ldots - (C + \log \Delta^{-1})\, I_0(x), \qquad (148)$$

comparing which with (140), we see that

$$(\log \Delta^{-1})\, I_0(x) = I_0(x) \log \frac{x}{2}; \qquad (149)$$

for which a verification would be desirable.

Companion Formulæ, $H_0(qx)$ *and* $K_0(qx)$, *derived from Companion Operators, expressed in Descending Series. Also in Ascending Series.*

68. Passing, however, at present to more manageable operators involving the two solutions and different forms thereof, it will be convenient to introduce a notation and standardization which shall exhibit the symmetry of relations most clearly. Thus, let

$$H_0(qx) = \frac{2\nabla}{(\nabla^2 - q^2)^{\frac{1}{2}}}; \qquad (150)$$

$$K_0(qx) = \frac{2\nabla}{(q^2 - \nabla^2)^{\frac{1}{2}}}. \qquad (151)$$

Here q is a constant and ∇ is d/dx. Superficially considered, these functions only differ in one being i times the other. But the common theory of the imaginary does not hold good here, or in operators generally. In a descending series we have

$$H_0(qx) = \epsilon^{qx}\left(\frac{2}{\pi qx}\right)^{\frac{1}{2}}\left\{1 + \frac{1}{8qx} + \frac{1^2 3^2}{\underline{|2}(8qx)^2} + \frac{1^2 3^2 5^2}{\underline{|3}(8qx)^3} + \ldots\right\}, \qquad (152)$$

as already shown. It is twice the function C, equation (3). Similarly, we may integrate (151). Introduce the factor ϵ^{-qx}, thus,

$$K_0(qx) = \epsilon^{-qx}\,\epsilon^{qx}\frac{2\nabla}{(q^2 - \nabla^2)^{\frac{1}{2}}} = 2\epsilon^{-qx}\frac{\nabla - q}{(2q\nabla - \nabla^2)^{\frac{1}{2}}}\,\frac{\nabla}{\nabla - q}$$

$$= 2\epsilon^{-qx}\left(\frac{\nabla}{2q - \nabla}\right)^{\frac{1}{2}}. \qquad (153)$$

Expand in ascending powers of ∇, and then integrate; then

$$\mathrm{K}_0(qx) = \epsilon^{-qx}\left\{1+\frac{\nabla}{4q}+\frac{1.3}{\underline{|2}}\left(\frac{\nabla}{4q}\right)^2+\frac{1.3.5}{\underline{|3}}\left(\frac{\nabla}{4q}\right)^3+\dots\right\}\left(\frac{2}{\pi qx}\right)^{\frac{1}{2}}. \quad (154)$$

$$= \epsilon^{-qx}\left(\frac{2}{\pi qx}\right)^{\frac{1}{2}}\left\{1-\frac{1}{8qx}+\frac{1^2 3^2}{\underline{|2}(8qx)^2}-\frac{1^2 3^2 5^2}{\underline{|3}(8qx)^3}+\dots\right\}. \quad (155)$$

Thus the function $\mathrm{K}_0(qx)$ only differs from $\mathrm{H}_0(qx)$ in the changed sign of qx, except under the radical. These are the most primitive solutions of the characteristic equation, and are useful as operators relating to inward and outward going cylindrical waves, as well as for numerical purposes. The function $\mathrm{K}_0(qx)$ is also expressed by

$$\mathrm{K}_0(qx) = \frac{2}{\pi}\left\{\frac{q^2x^2}{2^2}+\frac{q^4x^4}{2^24^2}\left(1+\tfrac{1}{2}\right)+\frac{q^6x^6}{2^24^26^2}\left(1+\tfrac{1}{2}+\tfrac{1}{3}\right)+\dots\right.$$
$$\left.-\mathrm{I}_0(qx)\left(\log\frac{qx}{2}+\mathrm{C}\right)\right\}. \quad (156)$$

By $\mathrm{I}_0(qx)$ here and later should be understood merely the ascending series

$$\mathrm{I}_0(qx) = 1+\frac{q^2x^2}{2^2}+\frac{q^4x^4}{2^24^2}+\frac{q^6x^6}{2^24^26^2}+\dots. \quad (157)$$

Transformation from $\mathrm{K}_0(qx)$ to the Companion Oscillating Functions $\mathrm{J}_0(sx)$ and $\mathrm{G}_0(sx)$, both in Ascending and Descending Series.

69. The connection between these functions H_0 and K_0 and the oscillatory functions is very important, but was in one respect exceedingly obscure to me until lately. Thus (157) and (156) are usually reckoned to be companion solutions (unless as regards the numerical factor). But if we take $q = si$ in (157), the function remains real, and becomes the oscillatory function, the original cylinder function of Fourier. Thus

$$\mathrm{I}_0(qx) = \mathrm{J}_0(sx) = 1-\frac{s^2x^2}{2^2}+\frac{s^4x^4}{2^24^2}-\frac{s^6x^6}{2^24^26^2}+\dots. \quad (158)$$

On the other hand, the same transformation in (156) makes it complex, on account of the logarithm. Thus, using

$$\log qx = \log six = \log sx + \log i = \log sx + \tfrac{1}{2}i\pi, \quad (159)$$

by the well-known formula for $\epsilon^{i\pi/2}$, we convert (156) to

$$\mathrm{K}_0(qx) = \mathrm{G}_0(sx) - i\mathrm{J}_0(sx), \quad (160)$$

where $J_0(sx)$ is the same as in (158), and $G_0(sx)$ is its oscillatory companion given by*

$$G_0(sx) = \frac{2}{\pi}\left\{-\frac{s^2x^2}{2^2}+\frac{s^4x^4}{2^2 4^2}(1+\tfrac{1}{2})-\frac{s^6x^6}{2^2 4^2 6^2}(1+\tfrac{1}{2}+\tfrac{1}{3})+\dots\right.$$
$$\left.-J_0(sx)\left(\log\frac{sx}{2}+C\right)\right\}. \qquad (161)$$

What is obscure here is the getting of only one oscillating function from $I_0(qx)$, and of two from $K_0(qx)$. In corresponding forms of the first and second solutions we should expect both oscillating solutions to arise in both cases. However this be, the transformation (160) is in agreement with the other form (155). For, if we make the change $q = si$ in it, we obtain the same formula (160), provided J_0 and G_0 are given by

$$J_0(sx) = \left(\frac{1}{\pi sx}\right)^{\frac{1}{2}}\Big[R(\cos+\sin)\,sx+Si(\sin-\cos)\,sx\Big], \qquad (162)$$

$$G_0(sx) = \left(\frac{1}{\pi sx}\right)^{\frac{1}{2}}\Big[R(\cos-\sin)\,sx+Si(\cos+\sin)\,sx\Big], \qquad (163)$$

where R and Si are the real functions of sx given by

$$R = 1+\frac{1^2 3^2}{\underline{|2}\,(8qx)^2}+\frac{1^2 3^2 5^2 7^2}{\underline{|4}\,(8qx)^4}+\dots = 1-\frac{1^2 3^2}{\underline{|2}\,(8sx)^2}+\frac{1^2 3^2 5^2}{\underline{|3}\,(8sx)^4}-\dots, \qquad (164)$$

$$S = \frac{1}{8qx}+\frac{1^2 3^2 5^2}{\underline{|3}\,(8qx)^3}+\dots = \frac{1}{i}\left(\frac{1}{8sx}-\frac{1^2 3^2 5^2}{\underline{|3}\,(8sx)^3}+\dots\right). \qquad (165)$$

Now here (162) is Stokes's formula for $J_0(sx)$, known to be equivalent to (158). And (163) shows that this kind of formula for the oscillating functions allows us to obtain the second solution from the first by the change of sin to cos and cos to $-$sin. The function $G_0(sx)$ of (163) may be shown to be equivalent to the $G_0(sx)$ of (161) by other means, and certainly verifications are desirable, because transformations involving the square root of the imaginary are sometimes treacherous.

Transformation from $H_0(qx)$ to the same $J_0(sx)$ and $G_0(sx)$. Explanation of Apparent Discrepancies.

70. Now as regards the changed form of the $H_0(qx)$ function of (152), there is a real and once apparently insurmountable difficulty.

* I have changed the sign of K_0 and G_0 from that used in my 'Electrical Papers' (in particular, vol. 2, p. 445), in order to make them positive at the origin.

We know that $H_0(qx)$ and $2I_0(qx)$ are equivalent, both analytically and numerically. Why, then, does the first become complex, whilst the second remains real when we take $q = si$? They cannot be both true in changed form. Thus (152) becomes (doing it in detail)

$$\left(\frac{2}{\pi qx}\right)^{\frac{1}{2}} \epsilon^{qx}(R+S) = \left(\frac{2}{\pi sx}\right)^{\frac{1}{2}} \frac{1-i}{\sqrt{2}} (\cos + i \sin) sx \,.\, (R - i \,.\, Si)$$

$$= \left(\frac{1}{\pi sx}\right)^{\frac{1}{2}} (\cos + i \sin) sx \,.\, \left\{ (R - Si) - i(R + Si) \right\}$$

$$= \left(\frac{1}{\pi sx}\right)^{\frac{1}{2}} \Big[R (\cos + \sin) sx + Si (\sin - \cos) sx \Big]$$

$$-i \left(\frac{1}{\pi sx}\right)^{\frac{1}{2}} \Big[R (\cos - \sin) sx + Si (\cos + \sin) sx \Big]. \quad (166)$$

That is, using the functions (162), (163) again, we have the transformation

$$H_0(qx) = J_0(sx) - iG_0(sx), \quad (167)$$

whereas $2I_0(qx)$ becomes $2J_0(sx)$. This was formerly a perfect mystery, indicative of an imperfection in the theory of the Bessel functions. But the reader who has gone through Part I and §§ 27, 28 of Part II will have little trouble in understanding the meaning of (167). The functions H_0 and $2I_0$, though equivalent (with positive argument), are not algebraically identical. To have identity we require to use a second equivalent form, so that, as in § 28,

$$H_0(qx) = I_0(qx) + \frac{2}{\pi} \left\{ \frac{1}{qx} + \frac{1^2}{q^3x^3} + \frac{1^2 3^2}{q^5x^5} + \dots + qx + \frac{q^3x^3}{1^2 3^2} + \dots \right\}. \quad (168)$$

In this form we may take $q = si$, and still have agreement in the changed form. We obtain the relation (167), provided that

$$G_0(sx) = \frac{2}{\pi} \left\{ \frac{1}{sx} - \frac{1}{s^3x^3} + \frac{1^2 3^2}{s^5x^5} - \frac{1^2 3^2 5^2}{s^7x^7} + \dots - sx + \frac{s^3x^3}{1^2 3^2} - \frac{s^5x^5}{1^2 3^2 5^3} + \dots \right\}. \quad (169)$$

As I mentioned before in § 22, this formula for $G_0(sx)$ may be deduced from formulæ in Lord Rayleigh's 'Sound,' derived by a method due to Lipschitz, which investigation, however, I find it rather difficult to follow.

We have, therefore, three principal forms of the first solution with q real and positive, viz., $I_0(qx)$, $\frac{1}{2}H_0(qx)$, and the intermediate form (168). We have also three forms of the oscillatory function $G_0(sx)$, viz., (161), (163), and (169). But we have only employed two forms

of $K_0(qx)$, and two of $J_0(sx)$, in obtaining and harmonizing the previous three forms. It would therefore appear probable that there is an additional principal formula for $K_0(qx)$, and another for $J_0(sx)$, not yet investigated.

Conjugate Property of Companion Functions.

71. The conjugate property of the oscillating functions is

$$J_0(sx)\frac{d}{dx}G_0(sx) - G_0(sx)\frac{d}{dx}J_0(sx) = -\frac{2}{\pi x}, \tag{170}$$

using the pair (162), (163), or the pair (158), (161). And, similarly,

$$H_0(qx)\frac{d}{dx}K_0(qx) - K_0(qx)\frac{d}{dx}H_0(qx) = -\frac{4}{\pi x}. \tag{171}$$

But, in the transition from (171) to (170) by the relation $q = si$, it is indifferent whether we take $H_0(qx) = 2I_0(qx) = 2J_0(sx)$, or else $= J_0(sx) - iG_0(sx)$. This conjugate property is of some importance in the treatment of cylindrical problems by the operators.

Operators with two Differentiators leading to H_0 and K_0 and showing their Mutual Connections compactly in reference to Cylindrical Waves.

72. The fundamental mutual relations of H_0 and K_0 are exhibited concisely in the following, employing operators containing two differentiators, say ∇ and q, viz.,

$$\frac{\nabla q}{(\nabla^2 - q^2)^{\frac{1}{2}}} \quad \text{and} \quad \frac{\nabla q}{(q^2 - \nabla^2)^{\frac{1}{2}}}. \tag{172}$$

Here it should be understood that either ∇ or q may be passive, when it may be regarded as a constant. But when both are active, there are two independent operands, one for ∇ and the other for q. In a cylinder problem relating to elastic waves, we may regard ∇ as being d/dr, where r is distance from the axis, and q as $d/d(vt)$, where t is the time, and v the speed of propagation. We have

$$\left.\begin{aligned}
[\mathrm{P}]\ldots\ \frac{\nabla q}{(\nabla^2-q^2)^{\frac{1}{2}}} &= q\mathrm{I}_0(qr), \ldots\ldots [a] \\
&= \epsilon^{qr}\left(\frac{\nabla}{2q+\nabla}\right)^{\frac{1}{2}} q = \tfrac{1}{2}q\mathrm{H}_0(qr), \ldots\ldots [b] \\
&= \epsilon^{-vt\nabla}\left(\frac{q}{2\nabla-q}\right)^{\frac{1}{2}} \nabla = \tfrac{1}{2}\nabla\mathrm{K}_0(vt\nabla), \ldots [c] \\
&= \frac{1}{\pi(r^2-v^2t^2)^{\frac{1}{2}}}, \ldots [d] \\
&= \frac{1}{\pi}\,\mathrm{I}_0(vt\nabla)\,\frac{1}{r}, \ldots [e]
\end{aligned}\right\}, \qquad (173)$$

where the letters in square brackets are for the purpose of concise reference. Similarly, we have this other set,

$$\left.\begin{aligned}
[\mathrm{Q}]\ldots\ \frac{\nabla q}{(q^2-\nabla^2)^{\frac{1}{2}}} &= \nabla\mathrm{I}_0(vt\nabla), \quad \ldots [\mathrm{A}] \\
&= \epsilon^{vt\nabla}\left(\frac{q}{2\nabla+q}\right)^{\frac{1}{2}} \nabla = \tfrac{1}{2}\nabla\mathrm{H}_0(vt\nabla), \ldots [\mathrm{B}] \\
&= \epsilon^{-qr}q\left(\frac{\nabla}{2q-\nabla}\right)^{\frac{1}{2}} = \tfrac{1}{2}q\mathrm{K}_0(qr), \ldots\ldots [\mathrm{C}] \\
&= \frac{1}{\pi(v^2t^2-r^2)^{\frac{1}{2}}}, \ldots [\mathrm{D}] \\
&= \frac{1}{\pi}\,\mathrm{I}_0(qr)\,\frac{1}{vt}, \ldots [\mathrm{E}]
\end{aligned}\right\}. \qquad (174)$$

The first set is usually, though not essentially, concerned with an inward-going, and the second set with an outward-going wave. The exchange of r and vt and of ∇ and q, transforms one set to the other, so that the proof of one set proves the other.

In obtaining $[a]$ from [P] we regard q as a constant, or at any rate, as passive for the time, expand [P] in descending powers of ∇, and integrate directly with the result $[a]$, as in § 13, equations (28), (29).

To obtain $[b]$, introduce the factor ϵ^{qr} to [P], and expand the transformed operator in descending powers of q, as in § 14, equations (30), (31).

To obtain $[c]$, we make q passive, and introduce the factor $\epsilon^{-vt\nabla}$. Then expand the transformed operator in descending powers of ∇, and integrate as in § 68, eqautions (153), (155) (only there the operator is q, making the case [C]).

Details concerning the above Relations.

73. As regards [d], it may be obtained from [a], [b], or [c]. These have not yet been done, so a little detail is now given. Thus, from [b] to [d]:—

$$\tfrac{1}{2}q\mathrm{H}_0(qr) = \frac{\epsilon^{qr}}{(2\pi r)^{\frac{1}{2}}}\left\{1+\frac{1}{8qr}+\frac{1^2 3^2}{\underline{|2}(8qr)^2}+\dots\right\}\frac{1}{(\pi vt)^{\frac{1}{2}}}$$

$$= \epsilon^{qr}\left\{1+\tfrac{1}{2}\frac{vt}{2r}+\frac{1\,.\,3}{2^2\underline{|2}}\left(\frac{vt}{2r}\right)^2+\dots\right\}\frac{1}{\pi(2vtr)^{\frac{1}{2}}}$$

$$= \frac{\epsilon^{qr}}{\pi}\frac{1}{(vt)^{\frac{1}{2}}(2r-vt)^{\frac{1}{2}}} = \frac{1}{\pi(r^2-v^2t^2)^{\frac{1}{2}}}. \tag{175}$$

In the first line we expand the function H_0; to get the second line we integrate with unit operand; and, finally, let ϵ^{qr} operate to get (175).

74. Next, from [c] to [d]:—

$$\tfrac{1}{2}\nabla\mathrm{K}_0(vt\nabla) = \frac{\epsilon^{-vt\nabla}}{(2\pi vt)^{\frac{1}{2}}}\left\{1-\frac{1}{8vt\nabla}+\frac{1^2 3^2}{\underline{|2}(8vt\nabla)^2}-\dots\right\}\nabla^{\frac{1}{2}}$$

$$= \frac{\epsilon^{-vt\nabla}}{\pi(2rvt)^{\frac{1}{2}}}\left\{1-\tfrac{1}{2}\left(\frac{r}{2vt}\right)+\frac{1\,.\,3}{2^2\underline{|2}}\left(\frac{r}{2vt}\right)^2-\dots\right\}$$

$$= \frac{\epsilon^{-vt\nabla}}{\pi}\frac{1}{r^{\frac{1}{2}}(2vt+r)^{\frac{1}{2}}} = \frac{1}{\pi(r^2-v^2t^2)^{\frac{1}{2}}}, \tag{176}$$

which needs no explanation, as the course is similar to the previous leading to (175).

75. As regards deriving [d] from [a], this may be done by harmonic decomposition, thus,

$$q\mathrm{I}_0(qr) = \mathrm{I}_0(qr)\frac{1}{\pi}\int_0^\infty \cos svt\,ds = \frac{1}{\pi}\int_0^\infty \mathrm{J}_0(sr)\cos svt\,ds, \tag{177}$$

the value of which is known to be (175). Conversely, we may evaluate the definite integral by turning it to the analytical form $q\mathrm{I}_0(qr)$, which may be done by inspection, and then integrating through the equivalent operator $\mathrm{H}_0(qr)\frac{1}{2}q$. But this definite integral is only one of several that may be immediately derived from the operators in (173), (174) by harmonic decomposition, and it will be more convenient to consider them separately in later sections along with applications and extensions of the preceding.

Cylindrical Elastic Wave compared with corresponding Diffusive Wave through the Operators.

76. The formulæ [*e*] and [E] are of a somewhat different kind, since the operand is the reciprocal of the independent variable. They are proved at once by carrying out the differentiations. Thus, for [E],

$$\mathrm{I}_0(qr)\frac{1}{vt} = \left(1+\frac{q^2r^2}{2^2}+\frac{q^4r^4}{2^24^2}+\dots\right)\frac{1}{vt}$$

$$= \left\{1+\frac{\lfloor 2}{2^2}\left(\frac{r}{vt}\right)^2+\frac{\lfloor 4}{2^24^2}\left(\frac{r}{vt}\right)^4+\dots\right\}\frac{1}{vt}$$

$$= \frac{1}{(v^2t^2-r^2)^{\frac{1}{2}}}. \qquad (178)$$

So, by [C] and [E] we have

$$\frac{\pi}{2}\mathrm{K}_0(qr)\,q = \mathrm{I}_0(qr)\frac{1}{vt} = \frac{1}{(v^2t^2-r^2)^{\frac{1}{2}}}. \qquad (179)$$

There is an interesting analogue to this transformation from K_0 to I_0 occurring in the theory of pure diffusion. Change the meaning of q from $d/d(vt)$ to $\{d/d(vt)\}^{\frac{1}{2}}$, that is, to its square root. Then we shall have

$$\frac{\pi}{2}\mathrm{K}_0(qr)\,q = \mathrm{I}_0(qr)\frac{1}{2vt} = \frac{\epsilon^{-r^2/4vt}}{2vt}. \qquad (180)$$

The quantity v is no longer a velocity, however. In the theory of heat diffusion it is the ratio of the conductivity to the capacity. This example belongs to cylindrical diffusion, and is only put here to compare with the preceding example, which belongs to the corresponding problem with elastic waves without local dissipation.

IX. "On a Failure of the Law in Photography that when the Products of the Intensity of the Light acting and of the Time of Exposure are Equal, Equal Amounts of Chemical Action will be produced." By Captain W. DE W. ABNEY, C.B., F.R.S. Received June 13, 1893.

It has been generally assumed that when the products of the intensity of light acting on a sensitive surface and the time of exposure are equal similar amounts of chemical action are produced, and with the ordinary exposures and intensities of light employed such, no doubt, is practically the case, and any methods of measurement hitherto practicable have been insufficiently delicate to discover any departure from this law, if such departure existed. In some recent experiments

however, I have discovered that this law breaks down under certain conditions, and I think the fact worthy the attention of those interested in the subject, since it is possible that these conditions may arise with other experimenters. Quite lately I have described the method of comparing the photographic value of sunlight with that of candle light ('Photographic Journal,' June, 1893), which was as follows:—A beam of sunlight, after three reflections from plain glass mirrors, was admitted through a narrow slit to sensitive bromide paper stretched round a drum of about 4 inches in diameter. The drum could be caused to rotate round its axis at any speed up to about sixty revolutions per second, by means of an electro-motor. A small exposure with this light was given to the paper during the rotation of the cylinder. Subsequently an amyl acetate lamp was placed in position at any convenient distance from the same slit, and a fresh portion of the same sensitive paper exposed to its action during a much longer period, the rotation being continued as before. The slit was next replaced by a small square aperture, of some $\frac{1}{2}$ inch side, and further portions of the same paper exposed to the amyl acetate light at the same distance, for varying but known exposures, with the drum at rest. On development the paper showed three images, a narrow band of deposit of the width of the slit caused by the sunlight, a second band of the same width due to the light from the amyl acetate lamp, and a third row of squares of varying blackness of deposit due to the different exposures given with the drum at rest.

If the width of the slit be accurately measured, the band formed by the amyl acetate lamp is evidently superfluous, supposing the usually accepted law to hold good under all circumstances, as by measuring the blackness, or rather want of whiteness, of the different squares, and using them as ordinates to the abscissæ which were the times of exposure, and drawing a curve through them, the blackness produced by the sunlight could be referred to that produced by the light of the amyl acetate lamp, and its equivalent value in terms of the latter light be calculated. The band of deposit produced by the amyl acetate lamp was introduced as a check, for its blackness could also be referred to the curve, and the width of the slit be calculated from it. On making such calculations I was surprised to find that in every case the calculated width of the slit was always considerably less than what it was in reality, the difference being far beyond that which would be caused by any error in the measurement. This led me to commence an investigation into the cause of this difference, and what has already been carried out is sufficient to show that there is a failure in the usually accepted law. It may be pointed out that if it held good the sum of any number of very short exposures should be equivalent to a single exposure for the same length of time.

The experiment which naturally suggested itself was to expose a sensitive surface to the action of the light of an amyl acetate lamp passing through a slit as before, the drum on which it was stretched being caused to rotate at high and low speeds, and also to place on the same paper a scale of exposures with the drum at rest. These were all developed together. An example of one of many experiments is given as an illustration.

The circumference of the drum with the paper stretched round it was 12·25 in. The width of the slit was arranged to be 0·012 in. The amyl acetate lamp was placed 2 ft. from the slit, and a rotation of 30 per sec. was given to the drum for one exposure and 1 per sec. for a second exposure. In the first case the time of exposure during each revolution was $\frac{0{\cdot}012}{12{\cdot}25} \times \frac{1}{30}$ sec., or about 1/30,000 sec.

The sum of the exposures during 20 min. was thus 1·176 sec.

In the other case the exposure was

$$\frac{0{\cdot}012}{12{\cdot}25}, \text{ or about } 1/1000 \text{ sec.,}$$

and the sum of the exposures was, as before, 1·176 sec. Thus the first individual exposures had only $\frac{1}{30}$ of the duration of the second exposures, though in the aggregate they were the same.

A scale of blackness was made on the same paper, through a square aperture, without shifting the lamp, the exposures being $\frac{1}{8}$, $\frac{1}{4}$, $\frac{1}{2}$, 1, 2, 4, and 8 sec. On developing it was apparent to the eye that the first band was much lighter than the second. The scale and blackness of the bands were measured accurately, and the times of exposure which had been given to each band, on the assumption that the law enunciated held good, were calculated and found to be for the first band 0·6 sec., and for the second band 0·91 sec., instead of 1·176 sec. which was really given in all. Another example is where the slit was opened to 0·11 in., and the time of exposure reduced from 20 to 10 min. It was found that in this case the exposures given on the same assumption were 3·7 sec. and 5·28 sec., the real exposure given being 5·36 sec. The last experiment shows that if the slit had been slightly wider or the rotation slower the law would have been approximately obeyed.

Another experiment was made by throwing an image of the crater of the positive pole of the electric light on a hole bored in a plate about $\frac{1}{20}$ in. in diameter by means of a lens, and allowing the emergent beam to fall on the slit and paper, the drum being made to rotate as before. The same kind of results were obtained.

As it might be thought that this difference was caused by some action other than chemical, another series of experiments was undertaken. In these different sensitive surfaces were employed in order

to eliminate any possibility of the effect being due to any phosphorescence of the paper, though none could be detected. Plates were held stationary and exposures made by admitting light to portions of them through slits of known angular aperture, cut in a disc which could be rotated at any desired speed. Similar results were obtained to those already described. The quickest rotation gave the least density. It may be remarked that the more sensitive a surface is to radiation the less marked are the differences observable for the same speeds of rotation. This is what might be expected.

As an outcome of the experiments so far made, it seems that when exposures less than 1/1000 sec. are made on a sensitive surface, and the source of illumination is an amyl acetate lamp (Von Altneck's) placed 1 ft. from the sensitive surface, the law quoted *ante* fails.

The question of a very low intensity of light acting and of the sensitiveness to different spectrum colours is now occupying my attention.

Addendum. July 4, 1893.

Since the above paper was read I have made an investigation into the question as to whether the foregoing law fails when feeble intensities of light are acting, and find that it does so signally. Sensitive surfaces were exposed in a Spurge sensitometer, in which there are thirty graduations of light admitted to different parts of the surface at the same time, the intensity of light being varied by its admission through apertures of varying size. The smallest aperture used was 1/256 of the largest, and an exposure lasting 2650 sec. was given to the former, whilst 10 sec. was given to the latter. It was found that the blackness produced by the two was very different, that produced by the light passing through the small aperture corresponding to an area of 1/600 of that passing through the largest aperture, if the law held good. The light employed was a large illuminated surface, which was equal to one amyl acetate lamp placed $6\frac{1}{2}$ in. away from the surface, without passing through the sensitometer.

As some persons might doubt the accuracy of this method, a different mode of experimenting was adopted in the next series made. An amyl acetate lamp was used, and portions of a sensitive surface were exposed at different distances from it, on the assumption that the squares of the distances gave a measure of the exposures necessary in order to produce equality of chemical action. In one experiment exposures were made at distances of 2, 4, 8, 12, 16, 20, and 24 ft.; the duration of exposure at 2 ft. being 10 sec., whilst for the last it was 24 min. The intermediate exposures were calculated on the same principle, a scale of blackness was also made by exposing

other parts for different times at a fixed distance from the light. As a result it was found that, on development, the deposit was greatest when the exposure had been made at 2 ft. and diminished for each successive distance. By applying the measures of the different blacknesses obtained at the different distances to the curve obtained by the measurement of the scale of exposures, it was found that the exposure at 24 ft. ought to have been prolonged by 4·3 times to give the same blackness as that at 2 ft., the other distances giving intermediate results. If the law held good, the actual blackness of deposit at 24 ft. would have been obtained had the same exposure been given at about 50 ft. Other experiments are in progress, but it seemed advisable, without waiting for their completion, to make this addition to the paper, to show that the law fails both when short exposures and also feeble intensities of light are in question.

X. "On the Displacement of a Rigid Body in Space by Rotations. Preliminary Note." By J. J. WALKER, F.R.S. Received May 19, 1893.

Having been led to study more particularly than, as far as I am aware, has hitherto been done the conditions of the arbitrary displacement of a rigid body in space by means of rotations only, the results arrived at in the case of the single pairs of axes seem to me of sufficient interest and completeness to warrant their being recorded.

A comparison of these results with those arrived at by Rodrigues in his classic memoir "Des lois géométriques qui régissent les déplacements d'un système solide dans l'espace . . . ." 'Liouville,' vol. 5, 1840, at once suggesting itself, it may be proper here to recall the substance of the latter, and show how far they fall short of the object I propose to myself. The case of displacement by successive rotations round a pair of axes is discussed in § 13 (pp. 395—396), where it is shown that (p. 390), "Tout déplacement d'un système solide peut être représenté d'une infinité de manières par la succession de deux rotations de ce système autour de deux axes fixes non convergents. Le produit des sinus de ces demi-rotations multipliés par le sinus de l'angle de ces axes et par leur plus courte distance, est égal, pour tous ces *couples* d'axes *conjugués*, au produit du sinus de la demi-rotation du système autour de l'axe *central* du déplacement, multiplié par la demi-translation absolue du système."

Then (p. 396) the converse of this theorem is affirmed, viz., that "*Tout déplacement . . . peut toujours provenir, d'une infinité de manières, de la succession de deux rotations autours de deux axes non-convergents* pourvu que *le produit. . . .*"

In this conversion of the theorem above, it is strangely over-

looked that a displacement is not defined by the direction of axis, and amplitude, of the resultant rotation, together with the magnitude of the component of the corresponding translation along that direction (for in this form the proof is given, the axis being drawn through one end of the common perpendicular to the particular couple in respect of which the theorem is demonstrated), since these elements are common to an infinity of displacements.

This being premised, the laws connecting pairs of axes by successive rotations round which a given displacement of a rigid body in space may be effected are as follows:—

If the first axis (ζ' ξ') is taken arbitrarily, say parallel to a given vector, ζ', and passing through the term of a second given vector, ξ', its conjugate is parallel to a vector (ζ), the side common to three quadric cones, the constants of which are functions of ζ', ξ', and the vectors defining the displacement.

Each of these cones, whatever the direction of ζ', passes through one of three fixed vectors.

The directions of the axes being fixed in accordance with the above conditions, the locus of either axis is a plane, the places of the axes in which are so related that the connector of the feet of perpendiculars on them from any fixed point generates a ruled quadric surface.

[The last three paragraphs have been altered (July 15) after a correspondence, since the reading of the note on 15th June, with which Professor W. Burnside, F.R.S. (who, however, is not responsible for any statement herein), favoured me; as the result of which he sent me a geometrical proof that one axis might in all cases be taken arbitrarily both in position and direction. On revising my analysis, I found that what I had taken as an equation of condition was reducible to an identity.]

XI. "On a Graphical Representation of the Twenty-seven Lines on a Cubic Surface." By H. M. TAYLOR, M.A., Fellow of Trinity College, Cambridge. Communicated by A. R. FORSYTH, Sc.D., F.R.S. Received June 13, 1893.

(Abstract.)

The converse of Pascal's well-known theorem may be stated thus: if two triangles be in perspective, their non-corresponding sides intersect in six points lying on a conic. An extension of this theorem to three dimensions may be stated thus: if two tetrahedrons be in perspective, their non-corresponding faces intersect in twelve straight

lines lying on a cubic surface. This theorem may be deduced from the equation

$$xyzu = (x+a\mathrm{T})\,(y+b\mathrm{T})\,(z+c\mathrm{T})\,(u+d\mathrm{T}),$$

where $\mathrm{T} = \alpha x+\beta y+\gamma z+\delta u$; and $a, b, c, d, \alpha\ \beta, \gamma, \delta$ are constants. The equations of twelve lines on the surface are evident.

This paper shows how the remaining fifteen straight lines on the surface may be obtained by means of nothing higher than quadratic equations, and determines which of these lines intersect each other.

The paper then proceeds to give a graphical method of representing all the intersections of the twenty-seven lines on a cubic surface by means of a plane diagram, which admits of many interesting transformations.

By the help of such diagrams some of the known relations of the twenty-seven lines to each other are deduced, and some theorems with respect to the lines, which it is believed are new, are established; for instance, the number of closed quadrilaterals, pentagons, and hexagons on the surface is determined, as well as the number of ways in which nine triple tangent planes can be drawn to pass through all the twenty-seven lines, and the number of ways in which twelve of the lines can be chosen, so that they are the intersection of two tetrahedrons in perspective.

XII. "Further Observations on the Shoulder Girdle and Clavicular Arch in the Ichthyosauria and Sauropterygia." By H. G. Seeley, F.R.S. Received May 25, 1893.

On January 18, 1892, I communicated to the Royal Society observations on the nature of the shoulder girdle and clavicular arch in Sauropterygia, which were read on February 18, and published in the Proceedings on June 25, 1892. These studies had grown out of the examination of new remains of Anomodont Reptiles, which I obtained in South Africa; and were the result of an endeavour to gain a knowledge of structures in which the shoulder girdle in extinct Reptilia admitted of detailed comparison with those materials. I had made examination of the same region of the skeleton in Plesiosaurs and Ichthyosaurs, and communicated the results to the Geological Society, which were published in the Journal of that Society in November and December, 1874.

In the paper of 1892 I endeavoured to correct, enlarge, or justify interpretations previously given. One aspect of this revision led to a controversial paper, challenging some points of interpretation which occur among the facts in my contribution. It is entitled "On the Shoulder Girdle in Ichthyosauria and Sauropterygia," by J. W. Hulke,

F.R.S., received by the Royal Society April 11, read May 12, 1892. All that was then before the Fellows is printed in the Proceedings for August 26, 1892, p. 471, and the paper was printed in No. 316 of the Proceedings. As my own paper had not been published when that by Mr. Hulke was read, and as his paper is entirely devoted to controverting my conclusions and discrediting the existence of evidence which is there figured, it is obviously based upon an imperfect knowledge of the facts. I should have been content to have left the vindication of the truths and ideas which I endeavoured to state to others, but that I had no opportunity of meeting the author's contentions, when the abstract of his paper was read; and because there are misconceptions of my meaning, some of which I should be glad to remove. The point of view taken by Mr. Hulke as the foundation for his criticisms is said to be mainly embryological work upon existing Reptiles and Amphibians, the exact relation of which to the extinct Ichthyosauria and Sauropterygia cannot be stated with precision, though all writers concede that the groups compared, Urodela, Anura, Lacertilia, are distinct orders; and I believe that the differences between them are too great to be expressed in this way. The embryology of Ichthyosaurs and Plesiosaurs being necessarily unknown, it seems to me that no sound interpretation of the obscure parts of their skeletons can be based upon such evidence; unless it is previously shown that there is a predominant affinity of the extinct organic type with the recent type to which it is compared. I should, therefore, attach less value than does Mr. Hulke to the embryological considerations which he adduces in relation to the identification of bones as being omosternal on the one hand, or clavicular on the other, and as determining the existence or absence of a precoracoid element in these extinct animals. Rather than import into discussion such hypothetical foundations for nomenclature of the bones of the skeleton in extinct animals, I prefer to trust to visible evidence of the relative position of the disputed bones, and to such comparisons with their condition in allied animals as may appear to justify inferences as to their true nature.

In discussing the shoulder girdle in Ichthyosauria, I have suggested that the conditions of the bones appear to indicate a precoracoid element, which was cartilaginous, and was not preserved. I assume that such an element may have extended from the scapula to the coracoid, transversely in front of the coracoid, and anteriorly between it and the clavicle. To this suggestion Mr. Hulke replies that the appearance of a division of the articular end of the scapula into three parts is fallacious (*loc. cit.*, p. 234) and illusory (p. 235). The basis for this statement is said to be a careful study of many Ichthyosaurian scapulæ, and especially of a separate scapula lent by Mr. A. N. Leeds, F.G.S. Mr. Hulke finds that the Oxford Clay scapula com-

prises in this region only two parts—"one posterior, glenoid, diarthrodial segment; the other, an anterior synchondrosial segment, which articulated with the coracoid." This is a point upon which I may state that in describing *Ophthalmosaurus** I suggested the view which Mr. Hulke has adopted. But I appeal from those disconnected bones to the evidence from the specimens in museums like the national collection. The Natural History Museum contains isolated scapulæ, but it also contains some scapulæ in natural position in the skeletons from the Lias, and there are other skeletons exhibiting the shoulder girdle in good preservation in the Geological Museum at Cambridge and elsewhere.

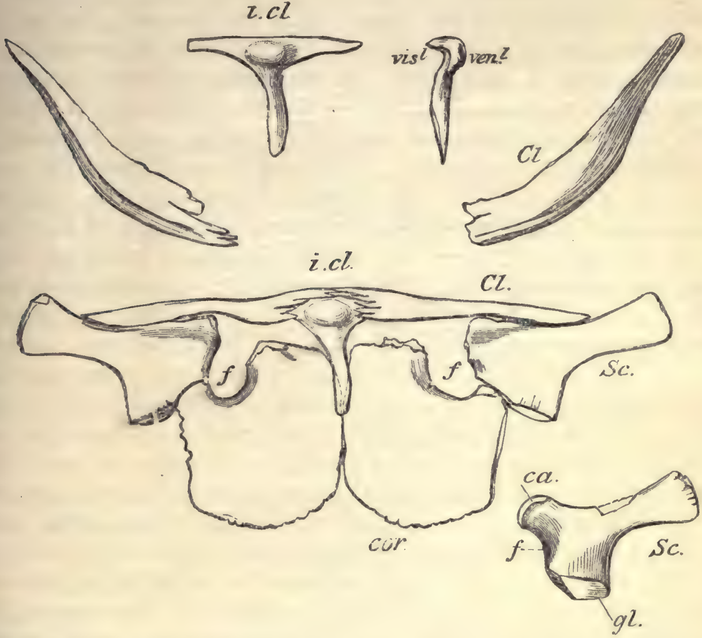


Fig. 1.—Shoulder girdle of *Ophthalmosaurus*. *i.cl.*, front aspect of interclavicle; on the right is a side view of this bone, showing its ventral and visceral contours. Beneath it are the posterior surfaces of the right and left clavicles (*Cl.*). Below this all the bones of the shoulder girdle are put together. The interclavicle is embraced by the clavicles; and (*cor.*) coracoid and scapula (*Sc.*) contribute to enclose the coracoid foramen (*f*). On the right and lower corner is an isolated scapula, with the margin of the coracoid foramen (*f*) completely ossified, preserving the cartilaginous surface (*ca.*). *gl.*, humeral articulation.

* 'Quart. Jour. Geol. Soc.,' 1874, vol. 30, pp. 693, 703. Pl. XLV, fig. 1, is a left coracoid. The surface lettered *c* is the humeral articulation; the surface *s* is the intercoracoid suture.

When the bones occur separate the scapula can usually be fitted to the coracoid; then the surface which formed part of the articulation for the humerus is clearly distinguishable from the surface which joined the coracoid. And if the antero-posterior extent, or measurement from within outward, of the coracoid surface which articulated with the scapula is taken, it will be found to be greatly exceeded by the length of the opposed surface of the scapula, as it extends from the humeral articulation forward to the clavicle. So that there is a free edge of the scapula, which is cartilaginous, extending in front of its coracoid articulation, and between that articulation and the clavicle. This surface is distinct from the coracoid surface, first in being much thinner; and, secondly, in commonly making an angle with that surface, though I do not attach much importance to the latter circumstance, as it may be affected by conditions of preservation and completeness of ossification. In one species of *Ophthalmosaurus*, in the British Museum, a part of this margin of the scapula anterior to the coracoid is concave and completely ossified where it formed part of the coracoid foramen (fig. 1, lower right-hand figure), but anterior to that is the unossified surface, which I suppose to have been for the precoracoid cartilage. On the inner anterior margin of the coracoid will also be found a surface, which indicates a cartilage, which I believe met the clavicle. The examination of the skeleton shows that the tripartite division of the scapula at its articular end, as figured by Cuvier, and as represented by Sir E. Home, to whom the sternal bones were first pointed out in detail by Buckland, as drawn by De la Beche for Sir E. Home, as represented in Professor Huxley's figure, and as affirmed by myself, cannot be regarded as illusory or fallacious on the evidence given; and at present no reference has been made to any skeleton from which such an inference could be drawn, or even in which the different condition affirmed by Mr. Hulke could be seen, though there is no reason why such a condition should not be found.

The importance of this discussion centres round the significance of the notch or concavity on the anterior border of the coracoid, which is placed towards the scapular margin. Does that notch represent the coracoid foramen of existing Reptiles? Such a foramen is seen in the coracoid bone in Lizards and *Hatteria*, and has been regarded as marking the union of the precoracoid and coracoid elements into one bone, and on that account I have spoken of it as the precoracoid foramen. It is more distinct in Amphibians, though it is differently placed. It occurs in Crocodiles. Among extinct animals it is found in the Saurischia and Ornithischia. It is present in the scapular arch of *Pareiasaurus* and Anomodonts. It may be compared to the foramen in *Ornithorhynchus* between the scapula and precoracoid. If the notch in the coracoid of *Ichthyosaurus*, which is towards the scapula, should be regarded as representing the coracoid foramen of any of

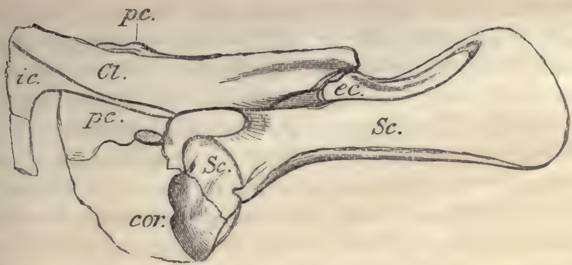


Fig. 2.—Left shoulder girdle of *Pareiasaurus Baini* as the bones were found before the matrix had been removed to separate the clavicular arch from the scapular arch. *ic.*, interclavicle; *Cl.*, clavicle; *pc.*, precoracoid; *cor*, coracoid; *Sc.*, scapula; *ec.*, epiclavicle.

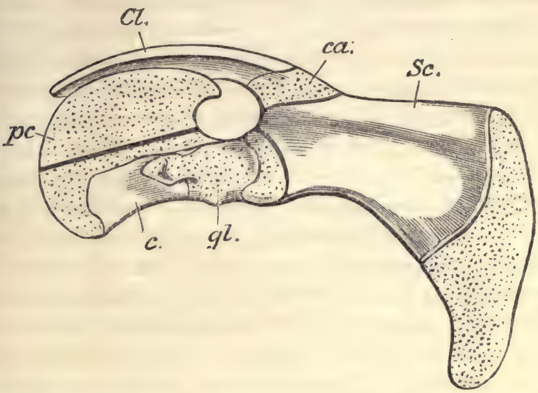


Fig. 3.—Shoulder girdle of a young *Ornithorhyncus*, after G. B. Howes, reduced and reversed. The dotted parts (*Ca.*) are cartilage; *Cl.*, clavicle; *pc*, precoracoid; *c*, coracoid; *Sc.*, scapula; *gl.*, humeral articulation.

these groups of animals, living or extinct, then it would, from the analogy of shoulder girdles which from age or plan are imperfectly ossified, be a legitimate inference, I submit, that the foramen which was defined on the one side by bone was completed on the other side by cartilage. The notch is in such a position that it is comparable to a coracoid foramen. No other determination for it has been suggested. If this identification were admitted, it seems to me highly probable, from comparison of the scapular arches in extinct Sauromorpha, that the cartilage which extended inward from the scapula was continuous with the cartilage which extended forward from the coracoid, and that the intermediate part defined the anterior margin of the foramen. If I understand Mr. Hulke, he would admit the existence of such a cartilage as an inference supported by analogy;

for he states, "the recess between the truncated antero-external corner of the coracoid and the adjacent antero-inferior angle of the scapula, both which parts bear, as Professor H. G. Seeley says, the mark of having had cartilage attached to them, is just the situation where a wider band of synchondrosial cartilage might be expected than was present posteriorly where the scapula and coracoid were nearer together. This passage seems to me practically to admit the point which I have affirmed, that the scapula shows a third cartilaginous attachment in addition to the two surfaces giving attachment to the coracoid and the humerus. Secondly, I urged that this cartilage probably connected the scapula with the inner truncated anterior border of the coracoid. No evidence is offered against this conception.

Then the only question of importance is by what name such a cartilage might be known. Mr. Hulke regards it as a persistent remnant of the *continuum* in which the bones originated. I prefer to name it precoracoid, because if it were a primitive cartilage which did not belong to either bone, it might be expected to be encroached upon by scapula or coracoid, or both; but during the whole period of time in which the genus *Ichthyosaurus* is found, there is no conclusive evidence of any such extension of ossification upon the scapula or coracoid. Neither scapula nor coracoid alter their forms at the expense of the supposed cartilaginous *continuum;* and, therefore, I infer that the cartilage was not ossified, but persisted as a precoracoid, though, as the coracoid foramen enlarged, the amount of cartilage left to represent it might become small. If the foramen enlarged so as to divide the cartilage into inner and outer portions, the external part adjacent to the scapula and coracoid would still be precoracoid, though the part adjacent to the inner anterior edge of the coracoid might assume the aspect of an epicoracoid. Such a separate condition of cartilages I understand Mr. Hulke to admit.

In my discussion of the shoulder girdle (*loc. cit.*, p. 120) it is remarked that I have failed to find "a specimen which leads me to doubt the substantial accuracy of the early interpretations of Home, Buckland, and Cuvier, in regarding the scapula as extending an articular surface inward and forward towards the pre-articular portion of the coracoid." This passage is referred to by Mr. Hulke in the following words: "In support of his conception of a precoracoid—cartilaginous—in Ichthyosauria, Professor H. G. Seeley cites the opinion held by Sir E. Home, Buckland, and Cuvier respecting the position and relations of the scapula." The two statements are not identical. Mr. Hulke reproduces the first of Sir E. Home's figures (p. 237) of the shoulder girdle, which I have known as "the Buckland figure," to distinguish it from the "De la Beche figure," given in the 'Phil. Trans.,' 1819, Pl. 14.

Home's first figure is spoken of as showing "unnatural proportions of the several bones." It is a generalised figure in which the scapulæ and the clavicles which rest upon them are about one-third too long, and the interclavicle is about twice as wide in the staff as any specimen which I remember (though, perhaps, not wider than in a specimen figured by Cuvier), but in substantial accuracy of arrangement of the bones the figure is admirable, and would be marvellous if made, as Mr. Hulke implies, from dissociated bones. Mr. Hulke does not point out any inaccuracy in the figure, which he reproduces, and no evidence is referred to which is opposed to the position of the scapula indicated in Pl. 2, fig. 1, 'Phil. Trans.,' 1818.

In the same way the criticism upon Cuvier seems to me without justification. It is stated by Mr. Hulke that, since "Cuvier copies both the figure given by Sir E. Home and the figure given by Dean Conybeare, and abstains from expressing his own views on the subject (of the difference between them), obviously little weight attaches to his authority in regard to it."

In the first place, Conybeare and Home agree in representing an anterior surface of the scapula in advance of the articulation of that bone with the coracoid. Secondly, Cuvier, in the plate in which Home's figure is copied, gives several admirable engravings of the shoulder girdle from specimens, and his fig. 1 and fig. 5 demonstrate that the scapula did extend a cartilaginous surface in advance of the articulation with the coracoid. A cast of the specimen represented in fig. 5 is preserved in the Natural History Museum. Cuvier's figures show variations in size of the anterior notch between the coracoid and scapula.

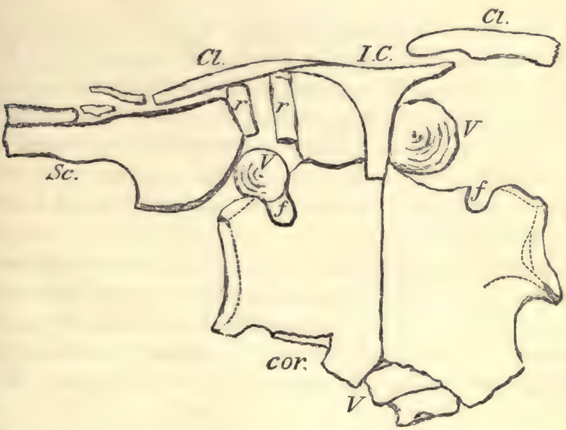


FIG. 4.—Shoulder girdle of *Ichthyosaurus*, after Cuvier, $\frac{1}{6}$ natural size ('Oss. Foss.,' Pl. 258, fig. 1). *I.C.*, interclavicle; *Cl.*, clavicle; *cor.*, coracoid; *Sc.*, scapula; *f*, coracoid foramen; *V*, vertebræ; *r*, ribs.

In fig. 4 it approaches nearer to the form of a foramen perhaps than in any other specimen, and in the interspace between the truncated anterior margin of the coracoid and the clavicle a surface is left smooth and distinct from the matrix in Laurillard's drawing, which may be only a hole in the matrix,* but that appearance possibly may be the foundation for the supposed epicoracoid described by the late Sir R. Owen in 1839 and 1866.

I fail to find any support for the critical position taken by Mr. Hulke, or for his restoration of the shoulder girdle of *Ichthyosaurus* ('Geol. Soc. Quart. Jour.,' 1883, p. 45), in the criticism which he makes of the authors referred to, who all take the view of anterior extension of the scapula in advance of the coracoid articulation;

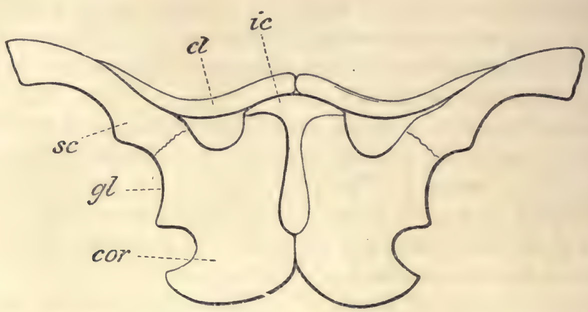


Fig. 5.—Mr. Hulke's restoration of the Ichthyosaurian shoulder girdle. *ic*, interclavicle; *ci*, clavicle; *sc*, scapula; *cor*, coracoid; *gl*, articulation for humerus.

while in Mr. Hulke's figure the breadth of the articular end of the scapula is made the same as the breadth of the surface of the coracoid with which it unites. This is at variance with every specimen known to me. The difference between Mr. Hulke, on the one hand, and other writers is not a matter of opinion or interpretation, but of fact, which can only be demonstrated by examination of specimens, or figures in detail of the structure, shown in all specimens which I have seen in skeletons well preserved.

I have stated that this identification of the precoracoid accounts for the structure of the shoulder girdle, and explains its homology. As both clauses of this statement are challenged, I may state, further, that by "structure" I mean the mode of arrangement of the bones by which the cartilaginous surface of the scapula extends forward in advance of

* Professor Albert Gaudry has had the kindness to examine this specimen for me, and has had the matrix partly removed so as to make the relations of the scapula and coracoid more evident. He finds no trace either of ossification or cartilage between the coracoid and clavicle.

the coracoid; and by "homology" I mean that, since the coracoid foramen is not in the middle of the coracoid bone, the Ichthyosaurian coracoid is not homologous with the Lacertilian coracoid; and, since this foramen in *Ichthyosaurus* is not defined by the coracoid and scapula, but could only be completed by a structure which occupied the open angle between the coracoid and scapula, the coracoid is not homologous with that of Saurischia or Ornithischia; and can only be compared with the coracoid of an animal in which a separate precoracoid is developed. I had not realised that this was a conclusion on which comparative anatomists have been in accord.

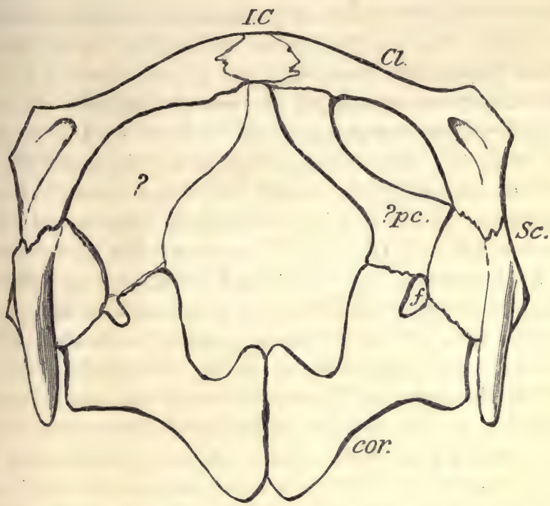


FIG. 6.—Shoulder girdle of *Nothosaurus mirabilis* restored. *I.C.*, interclavicle; *Cl.*, clavicle; *Sc.*, scapula; *cor.*, coracoid; *f*, coracoid foramen; *? pc.*, hypothetical cartilaginous precoracoid. On the left side this element (?) is represented as possibly extending along the clavicle, as in *Ornithorhynchus* and *Ichthyosaurus.*

I have also advocated the identification of the precoracoid as bringing the shoulder girdle of *Ichthyosaurus* into harmony with that of *Nothosaurus,* because in that genus there is a similarly incomplete coracoid foramen, and similar internal cartilaginous surfaces truncating the coracoid anteriorly and the scapula internally so as to include an angle between them, which such a cartilaginous precoracoid would occupy, so as to complete what I regard as the coracoid foramen. I only know the Nothosaurian shoulder girdle from von Meyer's excellent figures, and in contesting my interpretation I do not gather that Mr. Hulke has better knowledge of the original materials. It is urged that I am in error in identifying the coracoid foramen of

Nothosaurus with that of *Ichthyosaurus.* I have not proposed to identify it with the coracoid foramen in the coracoid of a Lizard, because I believe with Mr. Hulke that the precoracoid in Lizards is ossified. And it is because I find no evidence that the precoracoid is ossified in *Nothosaurus* (and I do not think there should be any contention that it is ossified, now that Mr. Hulke accepts the existence of clavicles in that genus) that I cannot regard the Lacertilian coracoid as homologous with the Nothosaurian coracoid. It is suggested by Mr. Hulke that the "coracoid foramen" in the Nothosaurian coracoid is not to be found in the small open notch which faces towards the scapula (*f*, fig. 6), but in the deep depression in the anterior contour of the coracoid, which is posterior in position, and nearer to the mesial line. This interpretation is founded upon Mr. Hulke's reading of the Lacertilian coracoid; but is unsupported by evidence, because the structures compared are morphologically different, and could only be brought into comparison, I submit, by first removing the precoracoid from the Lacertilian shoulder girdle, when the notch or foramen in the coracoid of the Lizard would face towards the scapula, as in *Nothosaurus*, with a similar open angle between the two bones. So far from the relations of what I regard as the cartilaginous precoracoid of *Nothosaurus* to the scapula and coracoid being different from what they are in *Ichthyosaurus*, they seem to me to be as nearly identical as possible in a widely divergent order of animals. For, in my conception, there is no reason why the notches which Mr. Hulke regards as representing the coracoid foramina should not entirely disappear under ossification of the anterior margins of the coracoid bones, so as to bring what are at present the two widely separated anterior processes of the coracoids into close union with each other, when the difference from *Ichthyosaurus* would be less apparent. As my meaning has not been clearly understood, I offer a restoration of the shoulder girdle of *Nothosaurus mirabilis*, showing what I conceive to be the position of the cartilaginous precoracoid.

This identification of the precoracoid foramen does not depend upon the evidence from *Nothosaurus* only. There are small unnamed Nothosaurs, figured by von Meyer in his 'Saurier des Muschelkalks,' Pl. XXXIII, fig. 45, &c., showing on the inner side of the scapula a notch with ossified margin (fig. 7, *f*), altogether distinct from the cartilaginous margin of the bone behind and in front of it, and therefore there is no doubt that both scapula and coracoid in those animals contributed to the formation of a foramen between those bones, which was completed by cartilage, as in the Ophthalmosaurs already referred to. The argument concerning that cartilage is in every respect the same as that offered in *Ichthyosaurus*. There is, however, this difference. The animal is fundamentally different in general organisation. Nothosaurs were for a long time included with the

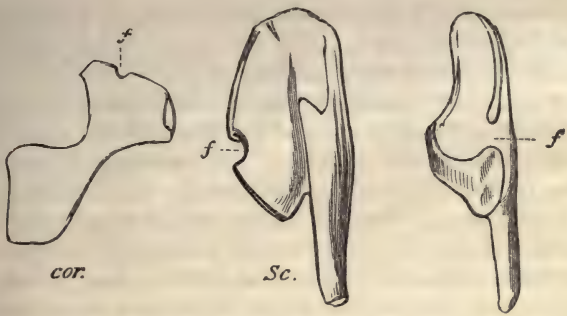


FIG. 7.—*cor.*, right coracoid ; *Sc.*, right scapula, visceral and internal aspects ; *f*, portion of the scapulo-coracoid foramen. (After von Meyer.)

Sauropterygia; but since 1882 it has been clear to me that they form a different order, which is intermediate between the Sauropterygia and the Anomodontia. And although it may not be possible at present to fully establish this conclusion, it is one in support of which evidence can be adduced. It is on this account that the interpretation of the shoulder girdle in *Nothosaurus* has appeared to me to have a two-fold significance as establishing, first, by comparison with *Plesiosaurus*, the true nature of the clavicles of Sauropterygia, and secondly, by comparison with the Anomodontia, I think it places beyond question the true nature of the precoracoid. The large questions of organic affinity I regard as safe bases for the morphological interpretation of the skeleton. If I do not enter into discussion of *Lariosaurus*, it is because the scapulæ are displaced, and I have already referred to the type on a former occasion.

The Anomodont comparison is important. First there is a notch in the Anomodont scapula which I regard as a Mammalian character, and this notch is completed externally by the ossified precoracoid, and passes obliquely through that bone, so as to excavate the coracoid. The relations of these bones are shown in *Pareiasaurus Baini* (fig. 2), and in many South African Anomodonts. As the specimens have been figured, it may be sufficient to refer to the figures as showing that the relations of the precoracoid to the coracoid and scapula are almost identical with those which I have suggested for the cartilaginous precoracoid in *Nothosaurus* (fig. 6) and *Ichthyosaurus* (figs. 1 and 4). Mr. Hulke has not pointed out any difference in this part of the skeleton from the Ichthyosaurian or Nothosaurian type, which would invalidate my interpretation that the difference between them, which is most essential in plan, is that Anomodonts have the precoracoid ossified. The ossification occupies substantially the same position which I have attributed to the precoracoid in the types in which it is supposed to

be cartilaginous. The weight of this comparison consists first in the direct resemblance of plan between the Anomodontia and Ichthyosauria in the clavicular arch and the shoulder girdle, where the difference is essentially that in the former the precoracoid is ossified, while in the latter there is a vacuity in the position which the precoracoid occupies in the former. And secondly, although the resemblance in detail in this region between the Anomodontia and the Nothosauria is less close, as shown in the construction of the scapula, there is a closer organic affinity between these types, which gives importance to the resemblances which have been stated.

The anatomical comparisons which have been made amount, I submit, to as close an approximation to proof that the precoracoid was represented by cartilage in *Ichthyosaurus* as could be given of a structure which is necessarily not preserved in the skeletons in which it has been argued to have existed. They may be thought to justify the suggestion of a cartilaginous precoracoid in *Ichthyosaurus* which was advanced in my paper.

The Sauropterygia.

I have regarded the Sauropterygian shoulder girdle as comprising the same bones as the shoulder girdle in Ichthyosauria and Nothosauria, and urge that the difference between them is that there is no trace of a precoracoid in Plesiosaurs, even the cartilage in the shoulder girdle indicated in those orders having disappeared. The clavicular arch in all three orders appears to me to be formed of the same elements, which I regard as being typically an interclavicle and two clavicles. These identifications are contested by Mr. Hulke, who advances the hypothesis that the bone which I regard as a scapula is a precoracoid in its inner portion and a scapula in its outer portion; and secondly, the hypothesis that the bones which I regard as a clavicular arch are not homologous with clavicular bones, but are a new kind of arch, formed from omosternal bones. Both of these hypotheses seem to me untenable, for the reasons presently to be stated. In the first place, attention may be directed to the precoracoid. Mr. Hulke has not explained why it is morphologically necessary to find a precoracoid in Sauropterygia when its existence is not affirmed by him in Ichthyosauria. He would apparently admit (fig. 4, *loc. cit.*, p. 241) that in Lacertilia the precoracoid loses its individuality by union with the coracoid, and, as I have stated, there are many examples which probably show such a condition among extinct animals. But here is a suggestion to blend the precoracoid with the scapula, to which no parallel can be found, as I believe, in true Reptiles, recent or fossil. It is not suggested by Mr. Hulke that any specimen exists in which there is a trace of a division of the

anterior bone of the Plesiosaurian shoulder girdle in the way which he represents by shading in his figure (fig. 8, p. 246) of the crushed and imperfect Woodwardian specimen, which may be compared with the figure given by myself in 1865 from a photograph, in the 'Annals and Magazine Nat. Hist.,' series 3, vol. 16, Pl. XV. In that specimen there is a partial longitudinal division, which I believe may be better explained by fracture. When the specimen was originally described, Plesiosaurian clavicles had not been identified, and I thought the division might represent a clavicle external to the scapula, and, although that view became untenable with the discovery of the clavicles in 1874 ('Quart. Jour. Geol. Soc.,' vol. 30, p. 444), I have since suggested that the ossification, if it ever were distinct, may represent the epiclavicle (*ec*, fig. 2) which extends along the superior margin of the scapula in *Pareiasaurus Baini*. It must be remembered that in *Plesiosaurus* this supra-arthroidal process of the bone is very thin, and ascends nearly vertically, so that it would be peculiarly liable to fracture. The specimen is elucidated by no other example in the separation and displaced position of the ascending process of the scapula; and, since it differs from other specimens from the Lias only in the horizontal and displaced position of that process, I have no doubt the specimen is delusive, in so far as it appears to suggest two separate bones. If the bones had been separate there would have been presumably a cartilaginous division line between the two elements, if both entered into the formation of the humeral articulation, in the position in which a division is figured by Mr. Hulke, whereas there is no such indication of division in the specimen, or in any other specimen.

This crushed bone is insufficient to support and sustain a new reading of the homologies of the great bones of the Sauropterygian shoulder girdle. If there is no other objective evidence in the Plesiosaurian skeleton, and Mr. Hulke mentions no other, there is, I submit, no evidence in support of a precoracoid in the Sauropterygia, except such as may result from comparison of Plesiosaurian bones with those of other animals, since the division drawn ('Roy. Soc. Proc.,' vol. 52, p. 246) is not in the line of fracture.

I am in entire agreement with Mr. Hulke in comparing the shoulder girdle of Sauropterygia with that of Chelonia, these orders being grouped in the Sauromorpha, in the scheme of classification given in 1891 ('Roy. Soc. Proc.,' vol. 49, p. 520). The difference between the views of Mr. Hulke and myself consists in the method of comparison and its results. In Sauropterygia the bone in advance of the coracoid which joins it by suture is in the same plane with the coracoid. In the Chelonia the bone which has the same relation to the coracoid is nearly vertical to the coracoid, or only inclined slightly forward. The Chelonian bone consists of two slender rays,

which diverge so as to include between them a large angle. The ventral ray extends transversely inward to the median ventral line towards its fellow, in a way to which no part of the bone in the genus *Plesiosaurus* offers a parallel; the dorsal ray ascends to the carapace in a way which is equally unparalleled in *Plesiosaurus*. There are two methods in which these structures may be compared in the two groups.

First, we may suppose, as Mr. Hulke does, that although the coracoids have no median union with each other in Chelonians, they are strictly comparable with the Plesiosaurian coracoids, which unite by a median suture. Then the ray of the anterior bone, instead of extending inward to meet its fellow, as in Chelonians, may be supposed to be directed forward to become an expanded plate, uniting with its fellow in some genera of Sauropterygia (*e.g.*, *Murænosaurus*) in the median line, and in such cases it may send a ray back to make a median union with the coracoid. Further, the vertical or forwardly inclined bar of the Chelonian bone is supposed in Plesiosaurs to be represented by the compressed plate which, ascending from the horizontal ray, extends above the articular surface for the humerus. Hence the two rays of the bone in Plesiosaurs are in two planes, one horizontal, and the other vertical, while in Chelonians the two rays may be regarded as substantially in the same plane, which, in so far as it is not vertical, is inclined forward. In both groups of animals Mr. Hulke names the ventral ray precoracoid, and the dorsal ray scapula.

There is another way of bringing the two types of shoulder girdle into comparison. If the elongation of the neck in *Plesiosaurus* is supposed to be brought about by an augmentation in number of the vertebræ from a type in which they were less numerous, then there is some ground for anticipating that the scapula, if originally in such a vertical position as it holds in Chelonians, would have its free superior end carried forward, until it might come to be in the same horizontal plane with the coracoid.* This is what I infer to have happened, and to represent the mutual relations of the parts, so that the forwardly directed horizontal plate of bone in *Plesiosaurus* would be homologous with the vertically directed bar of bone in Chelonians which all anatomists agree in naming scapula. And, therefore, there would be nothing in the Plesiosaurian shoulder girdle to correspond to the ray of bone which is directed inward ventrally in Chelonians, and that element, I suppose, to have practically disappeared from the Plesiosaurian skeleton. Hence the ascending ray of the scapula in *Plesiosaurus*, which extends above the humeral articulation, would not be homologous with the vertical ray of the scapula

* Some generic modifications of the Plesiosaurian pectoral arch, 'Quart. Jour. Geol. Soc.,' vol. 30, 1874, p. 439.

in Chelonians. There is in this interpretation an element of simplicity, because the scapula is simply inclined forward in a way to which there may be some slight approximation in Chelonians, which brings the two types of shoulder girdle into easy comparison. The coracoids in Plesiosaurs meet each other in the median line, and this condition has, I suppose, led to the atrophy and non-development of such a ray as Mr. Hulke terms the precoracoid, but which I believe to be a portion of the scapula,* on the hypothesis that such a ray may have once existed.

The choice between these methods of interpretation may depend upon the name adopted for the bone which most anatomists have termed scapulo-precoracoid in Chelonians, for if there is a precoracoid in the Chelonian, which is blended with the scapula, so as to be inseparable from it, then there would be some ground for Mr. Hulke's contention that the precoracoid was represented in the scapula of Plesiosaurs, even though there would be a difference of opinion still as to the position in which it was to be sought for, which would depend upon views as to the way in which the Plesiosaurian girdle was formed. Professor W. K. Parker termed the inner ray of the Chelonian bone precoracoid, and appears to base his interpretation chiefly upon the condition of the bones in the African Ostrich. It is stated ('Ray Soc.,' "Shoulder Girdle," p. 141) by the late Professor W. K. Parker, of *Chelone Mydas*, "There is nothing that can be called 'præ-' or 'meso-scapula,' save the swollen part in front, which passes uninterruptedly into the precoracoid; this front fork forms, with the scapula, a gentle arc (Pl. XII, fig. 3); it is of the same thickness, nearly of the same length, and has no separate osseous centre, the two bars being hardened by one ectosteal sheath." I gather that Professor Parker's observations were all made upon the ripe embryo or newly hatched young. A somewhat younger specimen in the Museum of the Royal College of Surgeons has by the kindness of Professor Stewart been examined for me by Mr. R. H. Burne, but without showing any indication of composite structure. Mr. Hulke quotes H. Rathke's account of the earlier ossification of this bone, in which it was found that each of the two limbs of cartilage has a distinct sheath, and that they had not quite reached each other at the stage described, in some types; while in another type the two limbs were united by ossification on their inner side. I can find no evidence in this condition that the bone which is termed precoracoid is distinct from the bone which is termed scapula, nor can I find in Rathke's account of the Chelonian shoulder girdle any evidence that he supposed that the two rays were distinct from each other, and that each contributed to the formation of the hollow of the shoulder joint. What Rathke describes is in complete harmony with the structure of every

* This view has also been adopted by Professor George Baur.

Chelonian bone, recent or fossil, which I have examined, so far as can be judged from the ossified bone, and it is closely comparable to the conditions of ossification of Plesiosaurs on the one hand, and of Amphibians on the other, in which the original cartilage of the humerus and femur becomes sheathed by an external layer of bone, leaving the cartilage in the position of an epiphysis, which penetrates more or less conically from the articular joint of the bone into the sheath, becoming an ossification which sometimes blends with it, and is sometimes for a time separate in the young state.

There is no evidence advanced that two distinct cartilages enter into the composition of the part of the glenoid fossa formed by this bone, and I affirm that its two rays are sheathed separately, because, owing to the original form of the cartilage, it would not be conceivable that the sheaths should be formed in any other way. But in his account of the young *Sphargis*, Rathke expressly states that the two sheaths coalesce when they meet each other, and therefore form one sheath, without any indication of a primitive separation between the two rays of ossification which afterwards became obliterated. Just as the ossification of the humerus of a Plesiosaur in three portions does not make that bone any the less a humerus, so the ossification of the scapula in a Chelonian in two or three parts, of which the articular part was originally cartilaginous and two are the coalesced ectosteal sheaths, does not seem to me to make that bone any the less a scapula in Chelonians. There is therefore no proof that the bone in Chelonians is anything but a scapula, or that the precoracoid is present in the Chelonian skeleton, any more than in the Plesiosaurian skeleton.

It only remains to point out that the ascending process of the Plesiosaurian scapula is homologous with that which is developed backward in the Nothosaurian scapula (figs. 6 and 7), and I have no doubt that both are homologous with the blade of the scapula in Ichthyosaurs and Anomodonts. But, in proportion as this scapular blade is developed, so does the scapula acquire a position which is posterior to the coracoid; while that position is the result of a special development of a new structure which ascends in these animals external to the ribs, above the humeral articulation. Its development is accompanied in Anomodonts by an atrophy of all that part of the scapula which had originally extended in advance of the articulation. This view may be hereafter established by further evidence. If the older view, which Mr. Hulke puts forward, should be preferred, there would still remain the evidence that the precoracoid has no existence in Chelonia, and therefore inferentially has no existence in Sauropterygia. So that the bone which extends in Plesiosaurs anterior to the coracoid would still be the scapula, and the shading which Mr. Hulke places upon it, dividing it into precoracoid and scapula, would be a delusive indica-

tion of an osteological separation of one bone into two bones which has no existence.

But the effect of the extraordinary difference in number of vertebræ in the several regions of the body in Plesiosaurs and Chelonians cannot, I think, be ignored in judging between the hypothesis of morphological displacement of one limb of the scapula as against morphological development of the other limb, when the bone is considered in Plesiosaurs from the point of view of a Chelonian comparison. The morphological resemblance with Chelonians is not so close as to solve the problem by comparison only.

Finally, there remains the clavicular arch, or, as Mr. Hulke terms the bones, omosternalia. Mr. Hulke regards the interclavicle as derived from the mesial ends of the clavicles (p. 246), and the omosternum as derived from the epicoracoids, so that in existing animals these structures appear to originate differently. Unfortunately there are no epicoracoids preserved in the Sauropterygia, so that it is impossible to base the nomenclature which Mr. Hulke prefers upon a morphological or structural basis.

The reason why the omosternal interpretation has been preferred by Mr. Hulke is stated (p. 252) to be the undisputed deep position of the bones, which are sometimes completely hidden by the scapulæ, and which always rest upon the visceral, as distinguished from the ventral, surface of those bones; and this, coupled with the composite structure, is the only reason advanced. I would compare this nomenclature for the bones, which I have figured as clavicular ('Roy. Soc. Proc.,' vol. 51, pp. 129, 131, 133, 140, 147, &c.), with the nomenclature adopted by Mr. Hulke for the corresponding bones in *Nothosaurus* ('Proc. Roy. Soc.,' vol. 52, p. 240). The interclavicle and clavicles are there represented, and the clavicles are correctly shown to extend on the deep-seated or visceral surface of what I regard as the scapulæ, precisely as in the Sauropterygia. So that the supposed proof from deep-seated position, which shows the bone not to be clavicle but omosternum in *Plesiosaurus* is exactly the same as that which is considered by Mr. Hulke to prove the bones which correspond to them in position in *Nothosaurus*, to be not omosternal, but clavicular. In both types the scapula is placed horizontally in advance of the coracoid, although there are some differences of form in the bones as compared with the Plesiosaurian genera. But all von Meyer's specimens show that the clavicles in *Nothosaurus* extend upon the visceral surfaces of the scapulæ, precisely as in *Plesiosaurus*. Mr. Hulke does not question that the bone named scapula in that genus represents the scapula. There is no suggestion that it includes the precoracoid; and it appears to be suggested that the part of the coracoid which is internal to the notch in the anterior part of that bone is precoracoid; so that the precoracoid would be an indivisible

portion of the coracoid. Hence, apparently, the difference in interpretation, the omosternum being assumed to join the precoracoid when it is present, while the clavicle is assumed to join the scapula. This interpretation is entirely hypothetical; for there is no more evidence in favour of the Nothosaurian coracoid being a coraco-precoracoid than there is in favour of the Plesiosaurian scapula being a precoraco-scapula. And therefore there is no foundation for the difference of interpretation which would name the bone which rests upon the visceral surface of the anterior element of the shoulder girdle in *Nothosaurus* a clavicle, and that which rests upon the visceral surface of the bone which occupies the same position in *Plesiosaurus* an omosternal bone. This arch in *Nothosaurus* consists of a small median piece and two long lateral pieces, which may, I think, be compared with the elements figured in *Plesiosaurus* ('Roy. Soc. Proc.,' vol. 51, p. 129), although the median element is much larger in the Plesiosaur, and the lateral elements are much shorter. Until some evidence is forthcoming to show that the bones which form an arch in the same position and are similarly situate on the visceral surface of the bones of the shoulder girdle are different, there is no justification for applying different names to them, or assuming that they are not homologous. In some of the smaller Nothosaurs figured by von Meyer it is evident that the anterior visceral surface of the scapula was smooth, so that the clavicle joined the scapula by squamose overlap, and not by suture, as in the genus *Nothosaurus*, thus approximating more nearly to the condition in *Plesiosaurus*.

The circumstance that the interclavicle in *Nothosaurus* and its allies is not wedged in behind the visceral surface of the coracoids, is necessarily a consequence of the small antero-posterior development of the median union between the coracoid bones, and the great length of the clavicles, by which the interclavicle is carried forward in the middle of an arch which is convex in front; while in Plesiosaurs what I believe to be the homologous arch is shorter and similarly directed backward. But there is no difference in plan. In Plesiosaurs (*loc. cit.*, p. 129) the clavicles are directed backward exactly as in *Nothosaurus*, and it is only the anterior margin of the interclavicle which is concave in front. There is so much in common in the structure of the skull, in the vertebral column, in the pelvic arch, the shoulder girdle, and limbs between the Sauropterygia and Nothosauria that there are probably no two other well differentiated orders of animals which have greater organic affinity with each other, and, therefore, although I have proposed to recognise a cartilaginous precoracoid in *Nothosaurus* of which no evidence is available in *Plesiosaurus*, I can see no ground for supposing that the bones of the shoulder girdle which are actually preserved are not severally the same as in the Sauropterygia.

There is another point in Mr. Hulke's argument. He adopts the classification of the shoulder girdle bones into primary or cartilaginous and secondary or membranous. There is no doubt that in the immature *Plesiosaurus*, in which all the indubitable cartilage bones show unossified cartilaginous surfaces and margins, the two bones figured ('Roy. Soc. Proc.,' vol. 51, p. 133) which I regard as clavicles are completely ossified, with sharp well-defined margins, and show no signs of immaturity; and I therefore regarded them as membrane bones. Some of the clavicular bones figured at the same time (*loc. cit.*, p. 131) are almost as thin as bones could be, and in marked contrast to the cartilage bones of the skeletons with which they are severally associated, although there are other examples in the collection of Mr. A. N. Leeds, of which he has since had the kindness to send me drawings, which are considerably thicker. If these bones had been omosternal bones, segmented from epicoracoids which are cartilages, presumably they would have been cartilage bones. But the immature specimens to which I have referred show no indication of having had a cartilaginous origin. Mr. Hulke states that it is possible that the bones which he terms omosternal are membrane bones, but adds that this is not yet absolutely certain, and yet, having urged that the distinction between the two groups of bones rests upon their different origin in the embryo, concludes that the weight of evidence is still in favour of an omosternal homology.

I am unable to imagine any evidence more conclusive than that which has been brought forward, based upon the condition of the bones themselves in the young Plesiosaur, and comparison with the condition in *Nothosaurus*.

From 1874 ('Quart. Jour. Geol. Soc.'), I have indicated affinities between Sauropterygia and other animals. The comparisons which have weight are indicative of a common plan between the animal types compared. Affinities which can thus be demonstrated may justify views of homology in interpreting obscure parts of the skeleton, which are more valuable than the views based upon resemblances of form found in isolated bones in animals which are widely different in organisation. I have formerly pointed out elements of the skeleton in which Plesiosaurs show characteristics of Amphibians, as in the mode of ossification of their long bones. Exactly the same condition of ossification is found in *Nothosaurus*. If this is an Amphibian inheritance which amounts to identity of plan in the construction of the limb bones, in a way which marks these two orders of animals off from other groups, it does not furnish an *a priori* ground for assuming that the coracoid, the scapula, and the arch of bones in front of these are all morphologically different in the Plesiosaurs and Nothosaurs, but rather that they are substantially the same.

If the significance of these Amphibian characters is further found, as I urge, in a sequence of affinity between the Sauropterygia, Nothosauria, and Anomodontia, we should be justified in anticipating that there might be a community of plan in the shoulder girdle of those groups which would enable homologous elements to be recognised. Until such comparisons fail, they cannot be disregarded.

The view which I have discussed in justification of that offered to the Royal Society in 1892 may be summarised in the statement that the Anomodont is a type in which the precoracoid is ossified; that in the Nothosaur the precoracoid has ceased to be ossified, but is represented by cartilage; while in the Plesiosaur the precoracoid cartilage appears to be lost. But with this change there is no change of plan in the clavicular arch, other than results from the different habits of the several orders of animals and the forms of the girdle bones with which the arch is associated.

XIII. "Researches on the Structure, Organisation, and Classification of the Fossil Reptilia. Part VIII. On further Evidences of *Deuterosaurus* and *Rhopalodon* from the Permian Rocks of Russia." By H. G. SEELEY, F.R.S. Received June 10, 1893.

(Abstract.)

The author endeavours to separate the Labyrinthodont remains, distinguished by having teeth anchylosed to the jaw, from such as belong to animals having a Theriodont type of dentition. The genera founded upon cranial fragments which show the Theriodont type are *Deuterosaurus*, *Rhopalodon*, and *Dinosaurus*. The skull in *Deuterosaurus* is described from new materials, which make known the structure of the palate and other cranial structures. The palate is of Plesiosaurian type. The back of the skull is a vertical plate, and the brain cavity rises in a long vertical tubular mass to the parietal foramen. The quadrate bones descend below the foramen magnum in a way that is best compared with Plesiosaurs.

The articular end of the lower jaw is identified among bones figured by von Meyer.

The skull of *Rhopalodon* is nearly complete, and has a general resemblance to the skull of the South African Dicynodont *Ptychognathus*. The orbit is defended with a sclerotic circle of bones. Whereas in *Deuterosaurus* there is only one molar tooth, in *Rhopalodon* there are apparently eight molar teeth, which have the posterior edge finely serrated.

The vertebræ are known from isolated and connected specimens which indicate a larger number than usual of rib-bearing presacral

vertebræ, which appear to be not fewer than nineteen, and may have numbered twenty-six. The sacral vertebræ are deeply cupped, and the sacral ribs are developed as in *Nothosaurus* and *Pareiasaurus*. The sacral ribs form part of the articular face of the first sacral vertebra. The pelvis is imperfectly known; the ilium is not so extended as in Dicynodonts, and conforms to the type of *Phocosaurus*, which is regarded as Theriodont. The pubis and ischium are united together on the Dicynodont plan, but are only moderately developed.

The scapular arch is completely known, and is formed of scapula, coracoid, and pre-coracoid as in *Dicynodon* and *Pareiasaurus*. The humerus and bones of the fore limb were relatively short, and only fragments have been preserved which appear to be referable to ulna and radius.

The hind limb is known from several examples of the femur, which resembles that of *Pareiasaurus* in the proximal end, but at the distal end is more like the type described as *Saurodesmus*.

The tibia is known from its proximal and distal ends; it has a general resemblance to that of *Pareiasaurus*, but is more slender. These types are regarded as constituting a distinct group, named Deuterosauria, which is in many respects intermediate between the Placodontia and Theriodontia, but in skull structure appears also to approach Nothosaurs and Plesiosaurs.

XIV. "The Menstruation of *Semnopithecus entellus*." By Walter Heape, M.A., Balfour Student at the University of Cambridge. Communicated by Professor M. Foster Sec. R.S. Received May 16, 1893.

(Abstract.)

The specimens used in the following investigation were collected in Calcutta in 1891.

The phenomena attending menstruation are grouped into four periods, and these are subdivided into eight stages:

A. Period of rest. Stage I. The resting stage.

B. Period of growth. Stage II. The growth of stroma. Stage III. The growth of vessels.

C. Period of degeneration. Stage IV. The breaking down of vessels. Stage V. The formation of lacunæ. Stage VI. The rupture of lacunæ. Stage VII. The formation of the menstrual clot.

D. Period of recuperation. Stage VIII. The recuperation stage.

The body of the uterus consists of an internal mucosa and external muscle layers. The mucosa is composed of uterine and glandular epithelium, blood vessels, a few radial muscles, and stroma. The

stroma has the appearance of embryonic mesoderm, the internuclear protoplasm is drawn out into very delicate processes forming a continuous network; there is no intercellular substance to be seen, and a few long radial fibrils are present during the resting stage only. It is a very primitive tissue.

Period A.

Stage I.—The uterine epithelium is a single row of cubical cells; its outer edge is sharply defined in section but the protoplasm of the base of the cells is continuous with the protoplasmic processes of the stroma.

The glandular epithelial cells are columnar; they rest on a basement membrane but have no sheath.

Round nuclei are embedded in the protoplasmic network of the stroma, which is evenly disposed for one-third of the depth of the mucosa, while below that a few radially arranged fibrils occur.

The blood vessels are small and fairly numerous.

Period B.

Stage II.—An increase in the number of the nuclei of the stroma by amitotic division and probably by fragmentation, causes swelling and increase of density in the upper third of the mucosa—hyperplasia. Owing to pressure the nuclei become fusiform. An enlargement of vessels takes place. No decidual cells are formed.

Stage III.—The mucosa is further swollen. The epithelium is stretched and becomes thinner. Hyperplasia of the vessels directly below the epithelium takes place and they are congested.

The size of many of the nuclei of the stroma is reduced.

Period C.

Stage IV.—Hypertrophy of the uterine epithelium of the stroma and of the walls of the vessels appears all over the mucosa: followed by degeneration in the superficial region, where the dilated, congested capillaries break down, the blood contained therein being extravasated amongst the stroma.

The degeneration is probably amyloid or hyaline, not fatty degeneration.

A considerable increase in the number of the leucocytes in the superficial vessels takes place.

There is no migration of leucocytes and no diapedesis of red blood corpuscles, but where vessels are ruptured a few leucocytes are swept out together with red blood corpuscles, into the surrounding tissue; many leucocytes, however, remain attached to the remnants of the walls of the broken-down vessels.

Stage V.—The extravasated blood now collects into lacunæ which are first formed within the stroma but gradually extend superficially, displace the intervening stroma elements and lie directly in contact with the epithelium.

The vessels in the deeper mucosa remain intact; there is no trace of diapedesis and no red blood corpuscles or leucocytes in the stroma in this region.

Stage VI.—The lacunæ increase in size. The uterine epithelium and superficial stroma shrivel up and exhibit signs of degeneration. The epithelium ruptures and the blood contained in the lacunæ is poured into the uterine cavity.

Stage VII.—Denudation follows. All the uterine epithelium, a portion of the glands and in some places a whole gland, and a depth of about one-third of the layer of the stroma is cast away, together with ruptured vessels, red blood corpuscles and leucocytes. Of these substances the menstrual clot is formed.

This is a severe, devastating, periodic action which is very remarkable.

A ragged surface is left behind and the remaining stroma contains, at or near the surface, masses of extravasated blood. In the deeper parts of the mucosa there is no further change.

Period D.

Stage VIII.—The recuperation consists of the re-formation of the epithelium, partly from the torn edges of the glands and partly by means of the transformation of stroma elements into flat epithelium; of the formation of new capillaries in the superficial region out of the stroma cells which surround the intercellular spaces in which the extravasated blood lies, and in the return of this reclaimed blood to the circulatory system; of the return of the vessels in the deeper mucosa to their normal size and consistency, and of the return of the stroma to the condition of rest (Stage I).

The new epithelium, at first flattened, becomes cubical, and new glands are formed from folds of this epithelium.

The numerous leucocytes left with the extravasated blood are returned to the circulatory system with the latter; they do not migrate, they do not form new tissue *in situ* or pus on the wounded surface.

Ovulation.

Out of the ovaries of forty-two specimens of menstruating *S. entellus* only two were found in which recent discharged follicles were seen. Such a result appears to be sufficient to warrant the statement that—

1. Ovulation does not necessarily occur during each menstrual period, and
2. That menstruation is not brought about by ovulation.

The two corpora lutea seen occurred in specimens of Stage III and Stage IV; during these stages the first great increase of the blood supply to the mucosa takes place, and it therefore appears possible that the increased supply of blood to the generative organs during the early stages of menstruation may possibly induce ovulation when a sufficiently ripe ovum is present in the ovaries; there is direct proof, however, that an ovum is not dehisced at each menstrual period.

Conclusion.

Recent observations show that periods of growth and degeneration occur in the mucosa of the bitch when rutting, but denudation is not described. There is good reason to believe that the period of growth is invariably present in the mucosa of rutting animals, and, as ovulation and rut are stated to be coincident, it appears highly probable that the period of growth during menstruation represents the preparation of the mucosa for the reception and retention of an ovum, while the degeneration period represents the result of failure to fertilise the ovum or failure of ovulation.

I venture to express the belief that the function of menstruation may be thus expressed, but I fail to find any evidence of the origin of menstruation.

Note.—Since the above was written, I have seen Marshall's book on 'Vertebrate Embryology' (1893), in which he also divides the phenomena of menstruation into four stages, identical with my four Periods A, B, C, and D. The arrangement was arrived at independently.

XV. "Studies in the Morphology of Spore-producing Members. Part I. Equisetineæ and Lycopodineæ." By. F. O. Bower, D.Sc., F.R.S., Regius Professor of Botany in the University of Glasgow. Received June 9, 1893.

(Abstract.)

Two preliminary statements have already been communicated on this subject ('Roy. Soc. Proc.,' vol. 50, p. 265, and vol. 53, p. 19), dealing with some of the observations made during work extending over more than four years.

The paper which I now submit to the Society includes the detailed statement of results acquired from the Equisetineæ and Lycopodineæ.

The first pages are devoted to the discussion of points of general morphology of the sporophyte, as it is seen in archegoniate plants, together with a sketch of the history of opinion as to the morphological "dignity" of the sporangia and their relation to the parts (usually sporophylls) which bear them. The position of Goebel is adopted, that sporangia are as much organs, *sui generis*, as are shoots, roots, &c., no matter where they may be seated. It is customary to assume that the ontogeny will serve as a guide to the history of descent in plants as in animals. As applied in detail to the sporophyte generation this assumption cannot be upheld: for the conclusions drawn from wide comparison would be directly antagonistic to such a history. The young sporophyte of a Fern first forms foliage leaves, stem, and roots; only after a considerable period are sporangia produced. On the recapitulation theory it would be concluded from this that the vegetative system was the first to appear, while sporangia were of subsequent origin, and it might further be held that sporophylls are metamorphosed foliage leaves. But the whole comparative study of the sporophyte of lower forms leads to the opposite conclusion; spore-production was the first office of the sporophyte, and if the lower Bryophyta really illustrate the mode of origin of the sporophyte, the production of spores preceded the existence of a vegetative system of the sporophyte, and was apparently a constantly recurring event throughout evolution. It must, therefore, be concluded that the history of the ontogeny does not truly recapitulate the history of descent as regards the neutral generation; the sporophyte is, in fact, an intercalated phase which has acquired vegetative characters. Comparative study of the Bryophyta leads to the conclusion that the whole vegetative region was the result of progressive sterilisation of potentially sporogenous tissues.

A brief review of the progress of this sterilisation as it has already been recognised among the Bryophyta is next given; it is pointed out that (*a*) the sterilisation may involve the whole thickness of the sporophyte, as in the formation of the seta, or (*b*) it may make itself apparent only in individual cells of the sporogonial head (elaters). It is important to note that Leitgeb concluded that in certain cases the latter might be massed together to form solid tracts of sterile tissue, such as the columella of the Anthoceroteæ. But though a considerable degree of vegetative advance may be traced in the Bryophyta, and correlated with progressive sterilisation, still they are clearly marked from vascular plants by two characters: (1) the absence of appendicular organs; (2) the single continuous archesporium.

There are, at least, three possible ways in which plants with numerous separate archesporia may have originated from plants of the Bryophytic type: (i) by branching (chorisis) of a sporogonial

head; (ii) by formation of entirely new archesporia, having no direct connexion by descent from pre-existent ones; (iii) by partitioning of a continuous archesporium; this might readily result from partial sterilisation and formation of septa. It has been one chief object of this investigation to see what evidence may be gathered from Vascular Cryptogams of one or of all of these modes of origin. The question has been first approached by examination of the "strobiloid" forms.

The frequent presence of synangia in eusporangiate Vascular Cryptogams suggests either coalescence accompanying reduction in a descending series, or partitioning by means of septa in an ascending series; the first question in connexion with such synangia will be whether in any natural sequence of Vascular Cryptogams the progression from a non-septate to a septate condition can be traced; or the converse. Though the facts at hand do not amount to an actual demonstration, the Lycopodineæ and their allies are believed to be an ascending series, and they are seen to supply important evidence. The series *Phylloglossum*, *Lycopodium*, and *Selaginella*, *Lepidodendron*, and the Psilotaceæ show natural affinities. In a paper shortly to appear in the 'Annals of Botany' I have drawn attention to the remarkable anatomical similarity which links the Psilotaceæ to *Lepidodendron*, while no one would doubt the kinship of the latter to other Lycopods. To this series *Isoetes* may be added, for, though the anatomical correspondence is not so close, I think its affinities with the Lycopods are nearer than with any other family.

As regards the sporangia, there can be no doubt of the homology of the sporangium of *Phylloglossum*, *Lycopodium*, *Selaginella*, and *Lepidodendron;* similarity of position, structure, development (not traced in *Lepidodendron*), and function all show this. Within the genus *Lycopodium* differences of detail have been observed analogous to such differences as would result in the production of more bulky sporangia, such as those of *Lepidodendron* and *Isoetes*, though it is true these differences are not so extensive. In these very large sporangia trabeculæ are found, as rods or plates of sterile tissue, which may project far upwards into the sporangial cavity (*Lepidodendron*), or may extend the whole way through it to the upper wall (*Isoetes*). In the latter case it has been shown by Goebel that the trabeculæ are the result of differentiation of a potential archesporium, part of which is sterilised and forms the trabeculæ. But these are at most only partial septa.

The next step is to the Psilotaceæ; and the first question is that of the real nature of the synangium in these plants. While Goebel and Juranyi look upon the sporangiophore as an abbreviated axis bearing two leaves, the synangium occupying its apex, Graf Solms, from external observation of *Psilotum*, maintains the older view, that the whole sporangiophore is of foliar nature with two lobes, while the

synangium is a growth from its upper surface. This latter view I am able to support from evidence of sections both of *Psilotum* and *Tmesipteris.* The latter shows the synangium to originate below the apex of the sporangiophore, and from its upper surface, in a manner very similar to the sporangium of *Isoetes.* The form of the young synangium resembles that of the sporangium of *Lepidodendron*, with which genus also there is extraordinary anatomical similarity. The position so close to the apex of the sporangiophore is peculiar, but it is to be noted that there is variety among other Lycopodineæ in the position of the sporangium. The septum is similar in its origin to the sporogenous masses, and is not at first distinguishable from them; in this respect it also resembles *Isoetes.* It would thus appear that the whole synangium is comparable in origin and position, in the broad lines of development, and in function to the sporangia of other Lycopods, *that is, a septate comparable with a non-septate body.*

Misgivings which may be felt in face of such a conclusion will be in great measure removed by the results of study of certain modifications to which the synangia are liable. *Tmesipteris* appears to be a variable plant as regards the form and structure of its synangia; there is, however, some method in its irregularities; smaller synangia of simpler form and structure are found at the limits of its fertile zones, while about the middle of it synangia have been found with three loculi, corresponding to those of *Psilotum.* Examination of those of simpler form shows *that they may be only partially septate, or the septum may be absent from the first.* I have been able to prove in young synangia of this type that *the tissue which would normally form the septum may be sporogenous;* this is exactly the converse of what has been proved by Goebel in *Isoetes*, and the conclusion which may be drawn is that *there is no essential difference between the tissue which will form septum or trabeculæ and that which will form spores, since they can mutually undergo conversion.*

It has already been shown by others that in *Psilotum* the number of loculi in the synangium may vary, being sometimes two, normally three, but occasionally four or five. In *Tmesipteris* it may be one, two, or three; and as there is no doubt of the homology of these within the Psilotaceæ, we may conclude *that in homologous parts the loculi may vary in number from one upwards.*

We may recognise within the species *Tmesipteris* a correlation of size to number of loculi; the smallest specimens have no septum, and these are produced at the limits of the fertile zone, where nutrition may be failing; those which are of normal size have two loculi: occasionally, when of large size and well nourished, as at the middle of the fertile zone, the loculi may be three. Here is illustrated in one species much the same sequence as is seen elsewhere for distinct genera, such as *Lycopodium, Isoetes, Lepidodendron:* where the

sporangium is small there are neither trabeculæ nor septa, the exigencies of nutrition, and perhaps also of mechanical strengthening, not being felt (*Lycopodium*): where the sporangium is large sterile bands of tissue are present; these appear as trabeculæ or incomplete septa in *Lepidodendron* or *Isoetes*, but as complete septa in the large synangia of *Tmesipteris*. To those who accept the homology of the synangium of *Tmesipteris* with the sporangium of other Lycopodineæ the probability of this will appear specially strong. Such facts as these and their theoretical bearing are discussed at length in the memoir: the opinion is finally expressed that progressive sterilisation and formation of septa are factors which will have to be taken into account in solving the problems of origin of vascular plants, and especially of their numerous sporangia: such formation of septa will have to be considered as one factor which may help to explain the origin of the simpler vascular plants from forms of some Bryophytic character, in which the sporogenous tissue was one continuous band.

In the course of the investigation it has become apparent that *it is not possible to give any strict topographical definition of the archesporium which shall apply for all Vascular Cryptogams.* This will not surprise those who have recognised that the position of the archesporium is not fixed for all Bryophyta, while, on the other hand, the first segmentations which lead to the definition of the archesporium in Vascular Cryptogams do not correspond to those in Phanerogams, where there is a definite dermatogen.

The memoir, of which this is a brief abstract of a few of the salient points, is still incomplete: it is intended shortly to treat the spore-bearing members of the Filicineæ from a similar point of view, and in preparation for this a considerable number of observations have already been made. In the meanwhile, it may be stated that the main lines of argument pursued above in treating the strobiloid forms will be found to be applicable also for the Filicineæ. The second part will also include a general discussion of the whole subject.

XVI. "On *Megaladapis madagascariensis*, an Extinct Gigantic Lemuroid from Madagascar." By C. J. Forsyth Major, M.D., For. Cor. Zool. Soc. Lond., &c. Communicated by Dr. H. Woodward, F.R.S., V.P.G.S., &c. Received June 14, 1893.

(Abstract.)

The subject of the present paper is a somewhat imperfect Mammalian skull, together with a right and left mandibular ramus, apparently belonging to the same specimen, discovered by Mr. J. T. Last (collector for the Hon. Lionel Walter de Rothschild), in a

marsh at Ambolisatra, on the south-west coast of Madagascar, beneath a stratum of a white clayey substance (shell-marl ?) from 18 in. to 2 ft. in thickness.

At first sight the skull appears to have no relation whatever with any known Mammalian group, either existing or extinct.

Its salient features are:—The enormous lateral development of the anterior inter-orbital portion of the frontals, extending over the small thick-walled orbits; a comparatively narrow and elongate post-orbital frontal region, separated by a slight contraction from the equally narrow parietal region; a thick and flattened sagittal and an equally strongly-developed occipital crest. The zygomatic arch is high, and projects moderately outwards.

The brain-case is comparatively small in size, low and short, and placed at a considerably higher level than the facial portion. This last is elongate, with its anterior portion more elevated than the posterior. The cranio-facial angle is extremely obtuse, as in most of the lower Mammals, but, whilst in these the angle is open downwards, in the present skull it opens upwards, owing to the fact that both the facial and the cranial portions are somewhat bent upwards, the former anteriorly, the latter posteriorly.

A striking general character is the remarkable pachyostosis of all the bones of the skull.

The sutures are in great part obliterated, which, together with the advanced wear of the grinding teeth, is indicative of the old age of the specimen.

The cranium, in its general physiognomy, approaches, on the one hand to the South American "Howlers" (*Mycetes*), on the other to some Marsupials, and particularly to the genus *Phascolarctos*.

These resemblances the author considers to be merely isomorphisms, and are not indicative of true parentage, which is to be sought amongst the Lemuroidea. Of this sub-order the present animal would form a much specialised and gigantic member, being approximately three times the size of the largest existing Lemurids.

In tracing out these affinities, the author relies in the first instance upon the conformation of the molars and premolars (the canines and incisors are not preserved), which approach closely to the grinding teeth of some Malagasy Lemurids of the genus *Lepidolemur*, and even still more to those of *Chirogaleus*.

It is further pointed out that, in its peculiar features, the skull itself only carries to an extreme, characters which are present, but in a much lesser degree and in varying gradations, in members of the Lemuroidea, both recent (Lemuridæ) and extinct (Adapidæ).

The diminutive size of the brain is viewed by the author, in this instance, as a degeneracy. He anticipates that crania of young specimens would present a striking approximation to existing

Lemurids, and probably most of all to *Chirogaleus*. These resemblances would consist in a more rounded cerebral cranium, in the brain-cavity being relatively much more voluminous, and possibly even absolutely so, than in aged specimens, and the facial portion more shortened. Especially it is to be expected that the post-orbital elongation of the frontals, an isolated feature in the Lemuroidea, will be absent in young specimens, as it is due solely to the development in the adult of aerial sinuses.

Passing to considerations of a more general nature, it is strongly to be insisted upon, as one of the results of this investigation, that the short cerebral and elongated facial portion of the cranium, universally accepted as a characteristic feature of the primitive Mammalian skull, are, on the contrary, indicative of a very specialised condition.

The author maintains that retrogressive evolution is more frequently to be met with amongst the Mammalia than has hitherto been admitted, "low" organisation being by no means always synonymous with "primitive" organisation.

In accordance with the conclusions arrived at, the author suggests the establishment of a new genus, *Megaladapis*, for the present fossil skull, as well as its collocation in a distinct family of the Lemuroidea.

Lemuroidea:
1. Adapidæ, *Adapis* (extinct).
2. Anaptomorphidæ, *Anaptomorphus*, *Necrolemur* (extinct.)
3. Lemuridæ (recent).
4. Megaladapidæ, *Megaladapis* (extinct).
5. Chiromyidæ (recent).
6. Tarsiidæ (recent).

As bearing upon the question of the geological age of *Megaladapis*, the author briefly reviews the contemporary Vertebrate fauna which has been brought to light by the explorations of Mr. Last, and the earlier researches of Grandidier, who (26 years ago) explored the same marsh at Ambolisatra. Some of the Mammalian remains obtained are too scanty to admit of accurate determination, but they are sufficient to indicate other not less strange members of this curious fauna. The following Vertebrata may be enumerated:—

MAMMALIA: *Megaladapis madagascariensis*, Major.
Hippopotamus Lemerlei, Grand.
Potamochœrus, sp.

AVES: *Æpyornis maximus*, Is. Geoff. St. Hilaire.
" *medius*, Milne-Edw. and Grand.
" *modestus*, Milne-Edw. and Grand.

EMYDOSAURIA: *Crocodilus robustus*, Grand. and Vaillant.

CHELONIA: *Testudo Grandidieri*, Vaillant.
" *abrupta*, Grandidier.

As to the geological age, the evidence obtained goes far to prove that this "sub-fossil" fauna existed at a comparatively very recent period, and that Man was contemporary with these Vertebrates. The evidence of various kinds may be arranged as follows:—

1. The very fresh aspect of all the remains, which are
2. Found in marshes and in recent alluvia (dunes), the formation of which is still in progress.
3. *Crocodilus robustus* is still existing in lakes in the interior.
4. Some of the bones bear traces of Man's handiwork.
5. The record of a monstrous animal, probably the *Hippopotamus*, is preserved in the legends of the natives.
6. Amongst the accounts—brief descriptions with native names—given by a trustworthy explorer of the seventeenth century, De Flacourt, are several which, on account of size and other characters assigned to them, cannot be identified with any animals actually existing in the island. One of the descriptions may possibly refer to *Megaladapis* and that of a bird, from the size of an Ostrich to one of the species of *Æpyornis*.
7. Remains of domestic cattle were found, together with the bones of the extinct forms of Mammals, &c.

On the other hand, evidence is adduced in support of the almost certainty that a *Tertiary* Vertebrate fauna will be, sooner or later, forthcoming in Madagascar, owing to the recent discovery of Tertiary lacustrine deposits in several different localities in that island.

XVII. "Some of the Effects and Chemical Changes of Sugar injected into a Vein." By VAUGHAN HARLEY, M.D., Teacher of Chemical Pathology, University College, London, Grocer Research Scholar. Communicated by GEORGE HARLEY, M.D., F.R.S. Received June 13, 1893.

(From the Physiological Institute, Leipzig.)

It being no longer doubted that sugar is essential to life, and it having been experimentally shown that the animal organism can form saccharine matter out of proteids, as well as carbohydrates, it becomes of importance to know what changes sugar undergoes in the body before its elements are finally eliminated.

According to present knowledge, it is considered that by the three chemical processes of hydration, oxidation, and reduction, the molecule of sugar is changed into larger and smaller molecular groups, and that it is in the breaking down and building up of these that heat and vital energy are developed. The basis of this belief rests on the observation that when sugar is artificially introduced into the

system its disappearance is accompanied by the liberation of heat and the development of muscular energy.

To discover the *modus operandi* of this is a matter of extreme difficulty, from the fact that when sugar is artificially introduced into the living circulation it is rapidly distributed to every organ of the body. In each of these it undergoes specific changes. So that, while the number of its derivatives are great, the amount of each is proportionally small. So small is it, indeed, of some of them, as to render their collection in sufficient quantity for the purposes of investigation next to impossible. However, by the advice and kind assistance of Professor C. Ludwig, I made the attempt, and the present communication, which I have the honour to lay before the Royal Society, embodies in brief the results of the research.

The methods adopted were briefly as follows:—As sugar artificially introduced into the veins is rapidly, and in different animals in varying proportions, eliminated by the kidneys, the ureters were ligatured so as to retain the sugar in the organism. In all cases where it was intended to kill the dogs within a few hours, the ligaturing was done from the front. Whereas, when it was intended to keep the dog alive, the operation was performed from behind, and the ureters tied by means of a ligature-staff, so that, at any given time, the ligatures could be removed without re-opening the wound. In order that all the animals should be under exactly the same conditions, in so far as their assimilative functions were concerned, no food was given to them during the previous twenty-four hours.

A 50 per cent. chemically pure grape sugar, dissolved in normal saline, was slowly introduced into the jugular vein. 10 grams of sugar to every kilo. of the dog's weight were the most usual quantities employed, and the whole introduced in about an hour.

After the necessary treatment of the various organs, the quantity of sugar was estimated by Allihn's method, the quantity of lactic acid by Drechsel's method, and the quantity of glycogen by Kulz's method. Alcohol, acetone, acetoacetic acid, β-oxybutyric acid, and ammonia were tested for in the usual way.

The following is a summary of the facts obtained:—

1. *Nervous Phenomena.*—It was noticed that large quantities of sugar gave rise, in dogs, to a series of symptoms pointing to nerve centre poisoning, their severity varying in different cases.

These nerve symptoms were generally preceded by, or attended with efforts at vomiting. But, from their stomachs being empty, the animals only brought up frothy mucus.

In some cases, the irritation of the nerve centres was only shown by muscular movements, causing a trembling of the skin. In other instances the muscular contractions were sufficiently severe to cause a quivering of the limbs, or even well-marked convulsions.

During the convulsive stage the pupils were contracted, although still reacting to light. The respirations were greatly increased. Around the mouth frothy mucus collects. The convulsions alternated with periods of rest, which gradually prolonged themselves until the dog seemed to be asleep. The convulsions sometimes were followed by profound coma, out of which the animal could not be roused. In some of the dogs the coma set in without any preliminary convulsive stage. Both the convulsive and comatose state always passed off when the ureters were loosed, while in some instances all the symptoms disappeared of their own accord.

10 grams of sugar per kilo. usually proved fatal to small dogs during the convulsion stage, from the respirations suddenly ceasing. Large dogs (of 20, or more, kilos. weight), showed only very mild symptoms, even after as much as 12 grams of sugar per kilo. weight were employed.

From the order of appearance and disappearance of the symptoms above alluded to, it seemed as if the sugar, by the breaking up of its molecule, yielded a poison or poisons, which, on being further transformed, became harmless.

It is worthy of note that none of these symptoms occur when sugar is absorbed from the intestines, no doubt for the same reason as they do not occur when only small quantities of sugar are injected into a vein; namely, because the intermediate products are never present at any one time in sufficient quantity to produce them.

2. *The quantity of sugar that remained as sugar in the different tissues* of the body was estimated at various periods of time after the completion of its injection.

(*a*.) The *quantity of sugar in the blood* at different periods of time after its injection into the jugular vein, the ureters being ligatured.

No. of experiment.	Time after completion of sugar injection.	Sugar in 100 parts of blood.
IX	Before injection	?
	1 hour after	?
	6 hours after	0·095
X	Before injection	0·112
	1 hour after	0·438
	3 hours after	0·126
	6 hours after	0·084
XI	Before injection	0·079
	Immediately after	0·676
	4 hours after	0·311
	6 hours after	0·118

From this it was seen that the quantity of sugar goes on diminishing after its injection, and that by the sixth hour it had reached (Exp. X) even a lower point than what it stood at after the twenty-four hours' fast.

(*b.*) *The quantity of sugar found in the liver*, as compared with that met with in the blood.

No. of experiment.	Time after completion of sugar injection.	Sugar in 100 parts of	
		Moist liver.	Blood.
IV	6 h. 20 m.	0·92	0·056
III	7 h.	1·72	0·025

The great difference observable between the quantity of sugar in the liver and in the blood shows how imperfect must be the passage from the liver into the general circulation of such a diffusible substance as sugar. This is all the more remarkable when we remember that the liver cells are surrounded not only by blood, but also by lymph, vessels.

(*c.*) *The Presence of Sugar in the matter vomited.*—As previously mentioned, several of the dogs vomited a quantity of mucus after the injection of the sugar.

In the cases in which the vomit was analysed it was found to contain sugar in small quantities.

(*d.*) *Quantity of Sugar in the Alimentary Tract.*—In order to ascertain if any of the sugar was eliminated into the alimentary tract, the contents of the stomach and intestines of three dogs were analysed. As all were in a fasting condition, contamination from food had not to be feared.

In only one case was sugar found in the stomach, and then only to the amount of 0·174 gramme. In no case was it found in the intestines. So it had, in all probability, been eliminated by the salivary glands and swallowed with the saliva. I am led to this opinion on account of Weyert having pointed out that when large quantities of sugar are injected into a vein, it is, in small quantities, eliminated with the saliva.

One may therefore, I think, consider that the alimentary tract does not eliminate sugar.

(*e.*) *Quantity of sugar in the œdematous exudations*, collected from the kidneys and upper part of ureters after their ligature.

No. of experiment.	Time after completion of sugar injection.	Quantity of fluid.	Quantity of sugar.	Sugar in 100 parts of	
				Œdematous fluid.	Blood.
		c.c.			
V	1 h. 45 m.	16	0·463	2·89	0·290
II	7 h.	4	0·104	2·60	0·064
I	7 h.	10	0·196	1·964	—
III	7 h.	11	0·105	0·96	0·025

Here it is seen that the quantity of sugar in the œdematous fluid, as was observed in the case of the liver, is greater than in the blood.

(*f.*) *Quantity of Sugar in the Urine.*—In order to ascertain if sugar passed out in the urine six or more hours after its injection, the ligatures were removed from the ureters in five of the cases and the urine passed during the following night collected. In one case only was there as much as 0·06 per cent. of sugar found in the urine. Consequently one may conclude that after sugar is injected and the ureters kept ligatured six or more hours, no sugar is eliminated in the form of sugar by the kidneys. The small quantity found in the one case could have been derived from the exudation which had accumulated in the kidneys and ureters during the time of ligature.

3. *Quantity of glycogen found in the liver and muscles* after the injection of sugar into the jugular vein.

No. of experiment.	Time after completion of sugar injection.	Glycogen in 100 parts of	
		Liver.	Muscle.
V	1 h. 45 m.	0·43	0·22
IV	6 h. 20 m.	0·52	0·39
I	7 h.	2·31	0·81
II	7 h.	1·75	0·09
III	7 h.	9·65	—

The results obtained from these five experiments show that the quantity of glycogen met with after the injection of 10 grammes of sugar per kilo. of dog's weight is well within the ordinary limits. Consequently one cannot say that any very appreciable amount of glycogen had been formed from the sugar that had been artificially introduced into the general circulation.

4. As it was thought possible that *lactic acid* might be one of the substances into which sugar split up in the organism, quantitative estimations were made of it in the blood and certain of the tissues.

(*a*.) *Quantity of lactic acid in the blood* at various times after the intravenous injection of sugar.

No. of experiment.	Time after completion of sugar injection.	Lactic acid in 100 parts of blood.
IX	Before injection	0·106
	1 hour after	0·112
	6 hours after	0·069
X	Before injection	0·071
	1 hour after	0·107
	3 hours after	0·104
	6 hours after	0·084
XI	Before injection	0·097
	Immediately after	0·134
	4 hours after	0·116
	6 hours after	0·110

It is here seen that a marked increase of the lactic acid takes place after the injection of sugar. Further, that after the fourth hour it decreases slower than does the percentage of sugar itself in the blood. In the following table the effects of this slower decrease is well shown.

No. of experiment..	XV.	XVI.	IV.	III.	VI.
Time after injection	4 hours.	5 hours.	6 hours.	7 hours.	25 hours.
Sugar per cent.	0·072	0·073	0·056	0·025	0·090
Lactic acid per cent.	0·087	0·082	0·090	0·090	0·130

These results give an average of 0·065 per cent. of sugar as against 0·096 per cent. of lactic acid in the blood. Whereas, in dogs on normal diet, Gaglio has shown the quantity of sugar in the blood was more than the quantity of lactic acid.

(*b*.) *Quantity of lactic acid in the liver and muscles* as compared with the amount in the blood after ligature of the ureters and the injection of 10 grammes of sugar per kilo. of dog's weight.

No. of experiment.	Time after completion of sugar injection.	Lactic acid in 100 parts of		
		Liver.	Muscle.	Blood.
V	1 h. 45 m.	0·339	0·171	0·135
IV	6 h. 20 m.	0·135	0·112	0·090
III	7 h.	0·031	0·011	0·090

The liver is thus seen to contain more lactic acid than the muscles, while, on the other hand, the blood sometimes contains more and sometimes less than both the liver and muscles.

From this it might be inferred that if the amount of a substance in any given part of the body yields a clue to the seat of its formation, it is probable that the liver is the principal seat of the lactic acid formation, as pointed out by Wyssokowitzsch.

(*c*.) *Quantity of lactic acid excreted in the urine*, the ureters having been ligatured for six or more hours after the injection of sugar into the jugular vein.

No. of experiment.	Ureters having been ligatured.	Time during which the urine was collected.	Quantity of urine.	Contained lactic acid.	
				Total quantity in grammes.	Per cent.
			c.c.		
XV	6 h.	First 12 hours	475	0·115	0·024
		Following 24 hours	600	0·062	0·010
VIII	6 h.	,, 12 ,,	230	0·173	0·075
XI	6 h. 40 m.	,, 24 ,,	650	0·571	0·088
V	9 h.	,, 12 ,,	162	0·562	0·346

From these results it appears that lactic acid is not only increased in the blood after the injection of sugar, but that it is at the same time excreted in the urine, whereas lactic acid is not known to occur in healthy dog's urine.

5. As *acetone, acetoacetic acid, and β-oxybutyric acid* are often found *in the urine* of diabetics, it was decided to examine it for these substances.

(*a*.) *Acetone and acetoacetic acid* were found to be present in all the five urines examined.

(*b*.) *β-Oxybutyric acid* was searched for in the urine both with the

polariscope and as α-crotonic acid, but could not, with any certainty, be demonstrated.

6. The *blood* was examined for *alcohol, acetone, and acetoacetic acid* by distilling it at a temperature of 102° C. by means of steam passed through it. The distillate being examined for alcohol and acetone. While in normal blood so treated neither alcohol nor acetone could be demonstrated, in the blood of dogs subjected to the sugar injection the presence of both of these substances was readily recognised.

Acetoacetic acid was searched for by acidifying the blood with sulphuric acid and then again distilling it. By this means its presence was also demonstrated in the blood of the sugared dogs.

7. As not only alcohol and acetone but likewise acetoacetic acid was found in the blood after the intravenous injection of sugar, it was decided to see how much *ammonia* could also be distilled off after adding magnesia to the blood. In the first place, as it was necessary to see if ligaturing the ureters caused an increase of the ammonia normally present in the blood. The blood of a dog in which the ureters alone had been ligatured was distilled, first immediately after and then a second time when the ureters had been ligatured five hours. The first blood yielded 0·025 per cent. and the second 0·024 per cent. of ammonia. So the ligaturing of the ureters had caused no increase in the quantity of ammonia present in the blood.

The same thing was then done with the blood from dogs before and after the intravenous injection of sugar.

Weight of dog.	Ammonia obtained from 100 parts of blood.	
	Before the sugar injection.	5 hours after.
kilos.		
29·5	0·025	0·034
27·5	0·018	0·017
5·0	0·042	—
3·8	0·039	—

It is thus seen the sugar injection had caused no increase in the quantity of ammonia in the blood.

It may be safely concluded that ammonia is not the cause of the nervous symptoms which followed the injection of 1 per cent. of sugar intravenously.

As these results again do not accord with those which follow on the injection of alcohol, we are led to suppose that they may be in some way or another connected with the presence in the blood of the abnormal substances, acetone and acetoacetic acid, which, as was seen, were not only found in the blood but likewise in the urine.

The possibility that the increase in the quantity of the lactic acid may likewise assist, if not be a material cause, in producing the nerve-symptoms, is also worthy of note.

That sugar, acting as sugar, was not their cause was shown by the symptoms not appearing until from a quarter to one hour after its injection. That is to say, not till the quantity of sugar present in the blood had already markedly decreased.

XVIII. "Experiments on Variola and Vaccinia." By S. Monckton Copeman, M.A., M.D. (Cantab.). Communicated by Professor M. Foster, Sec. R.S. Received June 14, 1893.

In the course of some experiments on the bacteriology of vaccine lymph, I was confronted by a difficulty in the practical testing of certain of the results of my work. Continuous experimentation on children being obviously out of the question, I was naturally led to turn my attention to the discovery if possible, of some one or more of the lower animals which, by reason of their passing through the various stages of vaccination and more especially variolation in a manner comparable with that witnessed in the human subject, might serve me for control experiments.

Little or no success having been obtained with the various domestic animals, I next turned to the monkey tribe on account of their similarity in many respects to man, although assured at the time that they were not susceptible to either vaccinia or variola. On putting the matter to the test, however, I found that this was not the case, the inoculation of vaccine and of variolous lymph having each of them given, in my hands, successful results in every instance in which I have tried it on the monkey (*Rhœsus*).

In the case alike of variola and of vaccinia, the local result of inoculation attains its acme (*quâ* vesiculation) in the monkey, as in the human being, about the eighth day. The first signs of reaction appear usually on the third day, by which time, if variolous lymph has been used, there is a distinct, though very thin, crust over the site of inoculation. By the fifth day vesiculation has generally commenced, this becoming more obvious up to the eighth day, though even then it is much less marked in variolous cases than in those which have been vaccinated, the difference being easily recognisable.

Later the vesicle gives rise to a pustule, by which time there is not infrequently considerable swelling of the skin and subcutaneous tissue and of the nearest lymphatic glands. The pustule gradually dries up, and a scab is formed which is more pronounced after vaccination than variolation, and which falls off some time during the third week, if the monkey has not picked it off before.

The chief difference noted between the effects resulting from the local inoculation of these two diseases, in the monkey, is that in the case of variola there is more or less of a crust from the first; that vesiculation is much less marked in variolation than in vaccination; that with variola about the ninth to the eleventh day a general eruption may appear which in some instances covers the whole surface of the body; and that the final scab at the site of inoculation is not so elevated in the variolated as in the vaccinated animal.

In both cases there is a rise of body temperature, which is more marked and longer sustained in variola than in vaccinia. After variolation it was noticed in several instances that the monkey suffered from diarrhœa, that its eyes were suffused, and that it was not as active as ususal. A peculiar odour was also noticed, quite distinct from the well-known smell of "monkey."

In no instance had the disease a fatal termination.

It was next determined to make trial as to the protection against small-pox afforded in the monkey by previous vaccination, and the protection against vaccination afforded by variolation. And I went on to compare the effect produced by the use of human and of calf lymph respectively.

As the result of numerous experiments, it would appear that the mutually protective power of lymph obtained from these three different sources when inoculated on the monkey is practically identical in all respects.

Experiments have also been carried out on the bacteriology of vaccine lymph, with the view especially of discovering what means of storage are best adapted for securing sustained purity of the lymph and the unimpaired manifestation for indefinite periods of the action peculiar and essential to its use.

I have previously shown* that there are three species of micro-organisms, one or other, or all, of which is almost universally to be found in every specimen of vaccine lymph examined. These are (1) *Staphylococcus albus*, (2) *S. pyogenes aureus*, (3) *S. cereus flavus*; of which the first is frequently to be found in the upper layers of healthy skin. Numerous other bacteria occur from time to time, among which the *Staphylococcus pyogenes* deserves special mention.

These "extraneous" organisms flourish in the various nutrient media employed for cultivating purposes, and also in vaccine lymph itself when removed from the body, causing, by their exuberant growth, the opacity which sometimes occurs in old lymph stored in capillary tubes. It appeared, therefore, not unlikely that in this way the growth of the specific organism, if such exists, might be superseded.

Obviously, therefore, it was necessary to devise, if possible, some

* 'Transactions of International Congress of Hygiene,' 1891.

means of treating vaccine lymph which should inhibit all "extraneous" organisms without injuring its potency for vaccination. To this end I first made trial of the method of fractional heating as suggested by Kitasato for the isolation of the tetanus bacillus. Although apparently successful in many instances, the desirability speedily became apparent of some method of readier application and requiring less delicate manipulation.

This I at length found in the admixture with the lymph of a definite proportion of glycerine prior to storage in the usual way.

Not only is lymph thus treated efficient as vaccine in the old sense of the word, but as time goes on, instead of losing its effect on inoculation, its potency actually becomes increased. Experiment shows also that in tubes filled with such diluted lymph, opacity does not apparently result. As I have previously stated, the glycerine inhibits the growth of, and after a longer or shorter interval kills off altogether, those aërobic bacteria which I have termed "extraneous." This effect may be demonstrated by making, from tubes of glycerinated lymph of equal age, a series of plate cultivations at gradually increasing intervals of time, together with control cultivations from tubes of untreated lymph. These results have recently been entirely corroborated by Sclavo, Chambon and Ménard, Straus, and other observers, such corroboration being the more valuable since it would appear that none of these observers were acquainted with the similar results at which I had previously arrived, and which were published* more than a year ago.

There can thus, I venture to think, be no doubt as to the superiority of the suggested method of lymph storage over the perhaps simpler method which up to the present time has been commonly employed in England. In Germany and elsewhere glycerine has been made use of for various reasons, but hitherto without knowledge, as far as I am aware, of the peculiar action exerted by it in the purification of the lymph.

The Society adjourned over the Long Vacation to Thursday, November 16.

Presents, June 15, 1893.

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"On the Geometrical Construction of the Oxygen Absorption Lines Great A, Great B, and α of the Solar Spectrum."

By GEORGE HIGGS. Communicated by R. T. GLAZEBROOK, F.R.S. Received February 20,—Read March 9, 1893.

In the early part of August, 1890, the photographic work of the normal solar spectrum which I had undertaken had been carried as far as great A, or the limit of visibility in the red, and to λ 8350, or beyond z, in the invisible regions.

During the two previous months of continuously dull weather, while classifying and comparing results, I was interested, on making a close examination of the head portion of the A line, to find the symmetrical construction, the rhythmical grouping, the harmonic order of sequence, and other characteristics of the B line repeated here in every detail.

These two bands, together with alpha, are composed of a number of doublets or pairs, which approach each other on the more refrangible side with uninterrupted regularity, finally crossing, and at the limiting edges of all three bands the three last pairs overlap each other.

The differences of wave-length between the components of pairs increase in the same order.

These and other properties, which will be referred to, are still more obvious in the trains or flutings.

From its holding an intermediate rank in each of its distinguishing characters I was induced to adopt B as a typical group in a geometrical representation, and to investigate the subject by means of rectangular co-ordinates.

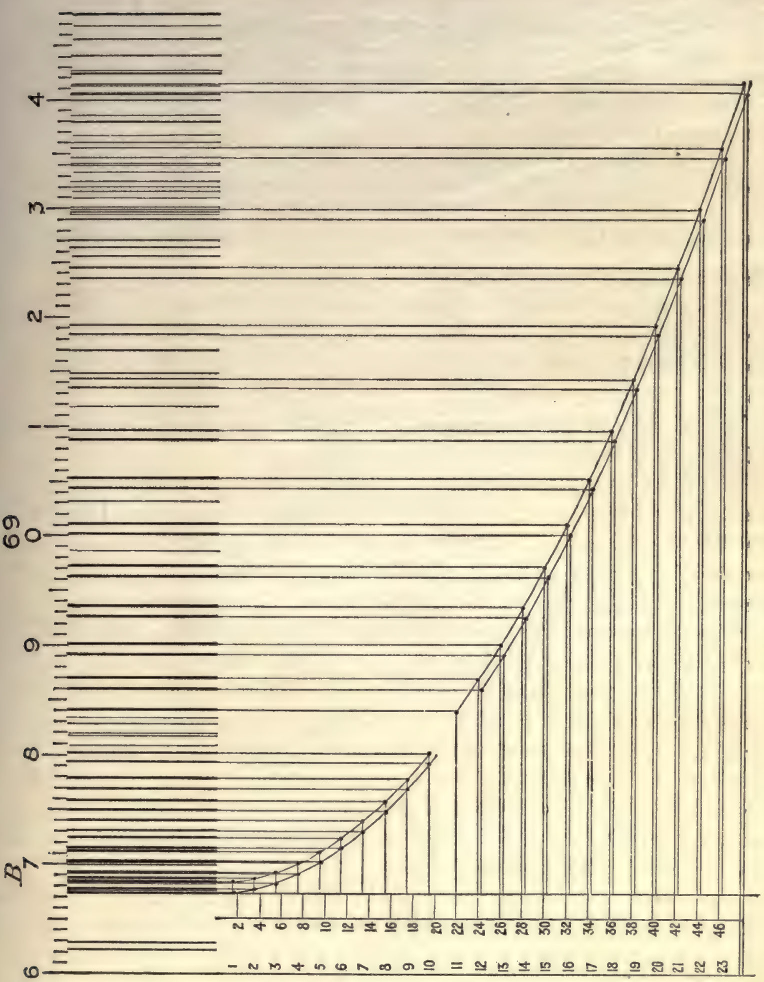
Before a complete analysis could be made out, a micrometer had to be completed. This consisted of a platform, serving as a plate holder, which was made to travel on runners between parallel ways by means of a screw of such a pitch as to move the negative from one division of the scale to the next, for one revolution of the divided plate on the screw head, this latter being divided into 100 parts.

On and over the platform, a microscope is mounted with slide motions at right angles to each other; an index of glass fibre and reflector complete the apparatus.

Over 1000 measurements of nearly 200 lines have been made, 100 of which belong to great A.

In the analysis the axis of x is assumed to occupy a position coincident with, or parallel to, the scale of $1/10^{10}$ m. units, and the positions of the various lines are set off on this scale (see fig. 1) for

FIG. 1.



the group, which is divided into four series. Ordinates are then drawn in the position occupied by each line. The axis of y is divided into a number of equal parts, 1, 2, 3, n. Lines parallel to the axis of x, drawn from each of these divisions, intersect the respective ordinates. The continuous curve passing through the points of intersection is found to possess all the properties of a parabola.

Three points at least are selected to determine the position of the vertex and value of latus rectum. The distance from the origin along y is also found for an ordinate to the first line of a series.

Now, from the equation to the parabola $y^2 = px$, the formula $\lambda = V + \frac{(n+c)^2}{p}$ is derived, where $V =$ the wave-length in $1/10^{10}$ m. units of a point in the spectrum coinciding with the vertex of the curve; p, the latus rectum; n, any number of units, reckoning from the origin; c, a constant.

In practice a representation more suitable for lantern projection being desirable, two units are taken on y for each line of the series; the equation then becomes $\lambda = V + \frac{(2n+c)^2}{L}$, where $L = 4p$, and c has twice its former value.

The computed places in the tables are derived from the equation in the latter form; the maximum want of agreement between these and the observed positions not exceeding (for α and B) 0·015 tenth-metre.

In the case of A the agreement is not quite so close, the maximum difference being about 0·05 tenth-metre.

It might be supposed that the greater difference arose from uncertainties of observation, caused by the greater haziness and breadth of the lines composing the A group; but it so happens that each component is in itself so much of a double as to show a bright rift in the centre, which facilitates the centralisation in some degree.

The differences referred to are attributable to the fact that the curve for any series in A, B, or α is not rigorously parabolic, but one which cuts the parabola in three points, similar to the curve of sines, cutting a straight line and terminating in the same phase as at the origin. This difference is so extremely minute in B (and in α still less) that it would require a representation more than 10 feet square, or a good sized lantern screen, to show two separate tracings at a point of maximum divergence, assuming the tracings to have but a breadth of 1/100th of an inch.

Following the stronger doublets in the fluting or train of A on the less refrangible side, is a secondary train of thinner, sharply defined, doublets, which, with a solar altitude of about 10°, may be traced on the photographic prints to about the 12th position. This series, which was not previously known to exist, conforms to the same formula, and in the table of wave-lengths is denominated the "Secondary Train of A." This secondary train follows in the wake of the right component of the primary series. In the head, however, similar secondary groups follow in the wake of both right and left components, overlapping and interlacing each other in such a manner that their resolution into series can only be arrived at by deductive processes; the difficulty is increased by the fact that a large number of positions are occupied by the dense lines of the main band.

These two series will be referred to as "Sub-groups" in the head

of A. They are, with two or three exceptions, given in a fragmentary state. At the same time, there is nothing to prevent their hypothetical positions being carried further, except that the greater density of the principal series precludes the possibility of obtaining any check in regard to their conformity.

Generally, a couple of numbers of the head bands are common to two separate series; this arises from their complexity being suggested by the nature of the analysis, and, as a matter of fact, some of these have been observed as doubles by Professor Rowland, of Baltimore.

In all cases of this kind a greater density is observable on the prints, and is doubtless the cause of the extra density of 7608·83, which belongs to two sub-groups; the line 7610·10 is known to be a double, but cannot with safety be measured as such.

Owing to their incompleteness, the elements of the curves for the sub-groups in head of A have not been made out, but a glance at their second differences is sufficient to establish their agreement with the preceding form, since an interval is equal to $d' + (n-1)\,d''$, where d' and d'' are first and second differences, and n any interval from the commencement of the series.

Note.—Since writing the above I find that Mr. Johnstone Stoney has written a note which was published with a paper by Dr. Huggins on the spectrum of hydrogen, in which he refers to the conditions under which members of a harmonic series might fall near to, but not on, a curve.

Fig. 2 is an enlargement of part of A

Fig. 2.

Head of the Alpha Line.

	1st Series.		2nd Series.	
	Computed.	Measured.	Measured.	Computed.
1.	6276·792	6276·798	6277·652	6277·644
2.	77·020	77·013	77·845	77·856
			78·190	
3.	77·514	77·518	78·280	78·335
		78·190	78·370	
4.	78·275	78·280	79·084	79·082
		78·370		
5.	79·302	79·302	80·095	80·095
6.	80·596	80·594	81·374	81·375
7.	82·156	82·148	82·924	82·922
8.	83·983	83·990	84·735	84·736
	V = 6276·775 L = 30·019 c = −1·29		V = 6277·632 L = 29·964 c = −1·41	

Train of the Alpha Line.

	1st Series.		2nd Series.	
	Computed.	Measured.	Measured.	Computed.
9.	..	..	6287·935	6287·942
10.	6289·596	6289·591	90·411	90·408
11.	92·344	92·350	93·140	93·141
12.	95·356	95·360	96·141	96·140
13.	98·634	98·640	99·416	99·407
14.	6302·176	6302·178	6302·941	6302·940
15.	05·984	05·980	06·741	06·740
16.	10·056	10·040	10·795	10·806
17.	14·394	14·399	15·135	15·140
18.	18·996	19·008	19·750	19·740
	V = 6276·693 L = 30·19 c = −0·263		V = 6277·746 L = 29·985 c = −0·515	

Head of Great B.

1st Series.			2nd Series.	
	Computed.	Measured.	Measured.	Computed.
1.	6867·464	6867·455	6868·457	6868·464
2.	67·776	67·788	68·782	68·771
3.	68·338	68·337	69·330	69·326
4.	69·150	69·148	70·130	70·130
5.	70·212	70·220	71·180	71·182
6.	71·523	71·530	72·485	72·484
7.	73·084	73·080	74·039	74·033
8.	74·895	74·892	75·834	75·831
9.	76·955	76·950	77·879	77·877
10.	79·266	79·274	80·170	80·172
	V = 6867·394 L = 32·03 c = −0·5		V = 6868·397 L = 32·194 c = −0·53	

Train of Great B.

1st Series.			2nd Series.	
	Computed.	Measured.	Measured.	Computed.
11.	..	..	6884·077	6884·090
12.	6886·012	6886·000	86·998	86·990
13.	89·181	89·182	90·140	90·142
14.	92·601	92·615	93·560	93·545
15.	96·271	96·277	97·200	97·201
16.	6900·192	6900·193	6901·120	6901·108
17.	04·364	04·368	05·264	05·267
18.	08·786	08·786	09·680	09·678
19.	13·458	13·444	14·334	14·340
20.	18·382	18·367	19·245	19·255
21.	23·555	23·545	24·412	24·421
22.	28·980	28·980	29·840	29·839
23.	34·655	34·662	35·518	35·509
24.	40·580	40·580	41·430	41·431
	V = 6867·529 L = 31·922 c = +0·29		V = 6868·812 L = 31·767 c = +0·03	

Head of Great A.

	1st Series.		2nd Series.	
	Computed.	Measured.	Measured.	Computed.
1.	7593·980	7593·98	7595·26	7595·260
			95·42	
2.	94·276	94·28	95·54	95·543
			95·66	
3.	94·796	94·79	96·05	96·050
		95·42		
4.	95·540	95·54	96·78	96·781
		95·66		
5.	96·508	96·49	97·73	97·736
6.	97·700	97·69	98·90	98·915
7.	99·116	99·12	7600·29	7600·318
8.	7600·756	7600·80	01·96	01·945
9.	02·620	02·64	03·77	03·796
10.	04·708	04·74	05·90	05·871
11.	07·020	07·03	08·21	08·170
12.	09·556	09·54	10·71	10·693
13.	12·316	12·31	13·44	13·440
14.	15·300	15·30	16·39	16·411
	V = 7593·904		V = 7595·195	
	L = 35·714		L = 35·715	
	c = −0·357		c = −0·473	

Train of Great A.

	1st Series.		2nd Series.	
	Computed.	Measured.	Measured.	Computed.
15.	..	..	7621·260	7621·299
16.	7623·590	7623·535	24·765	24·772
17.	27·310	27·310	28·480	28·480
18.	31·255	31·275	32·445	32·413
19.	35·425	35·460	36·59	36·571
20.	39·820	39·840	40·97	40·954
21.	44·440	44·470	45·57	45·562
22.	49·285	49·305	50·39	50·395
23.	54·355	54·360	55·448	55·453
24.	59·650	59·615	60·715	60·736
25.	65·170	65·148	66·218	66·244
26.	70·915	70·880	71·945	71·977
27.	76·885	76·840	77·89	77·935
28.	83·080	83·025	84·075	84·118
29.	89·500	89·450	90·49	90·526
30.	96·145	96·105	97·13	97·159
31.	7703·015	7703·020	7704·02	04·017
32.	10·110	10·160	11·16	11·100
	V = 7594·669		V = 7596·044	
	L = 35·556		L = 35·556	
	c = +0·067		c = −0·04	

Secondary Train of Great A.

1st Series.			2nd Series.	
	Computed.	Measured.	Measured.	Computed.
15.	7622·076	7622·06	..	7623·290
16.	25·613	25·62	7626·79	26·790
17.	29·356	29·36	30·50	30·502
18.	33·305	39·29	34·42	34·426
19.	37·460	37·46	38·57	38·560
20.	41·821	41·81	42·91	42·910
21.	46·388	46·36	47·46	47·470
22.	51·161	51·19	52·24	52·242
23.	56·140	56·14	57·23	57·226
	V = 7593·4535 L = 38·835 c = +3·34		V = 7596·122 L = 37·736 c = +2·019	

Sub-group in Head of A following the 1st Series. Fragment of Sub-group in Head of A following the 2nd Series.

Measurements only.			Measurements only.		
Sub-series No. 1.		Sub-series No. 2.	Sub-series No. 3.		Sub-series No. 4.
5.	7597·00	7598·20*			
6.	98·29	99·45			
7.	99·74	7600·90*			
8.	7601·42	02·57*			
9.	03·25	04·40			
10.	05·36	06·48	10.	7606·48	
11.	07·65	08·83	11.	08·83	7610·10*d*
12.	10·10*d*	11·28	12.	11·45	
13.	12·84	13·98	13.	14·28	
14.	15·78	..	14.	17·25 ?	

The numbers marked with an * are hypothetical positions.

"On the alleged Increase of Cancer." By GEORGE KING, F.I.A., F.F.A., Hon. Sec. Institute of Actuaries, and ARTHUR NEWSHOLME, M.D., M.R.C.P., Medical Officer of Health for Brighton. Communicated by Dr. J. S. BRISTOWE, F.R.S. Received February 27,—Read May 4, 1893.

During the last few years the minds of medical men and of the general public have been exercised over the rapid and striking increase in the mortality from cancer, as shown by the statistics contained in the Registrar-General's Annual Reports. The following table, taken from these reports, shows how great this increase in registered mortality has been. The registered death-rate of males from this disease was 2·7 times, for females 2·0 times, and for both sexes together 2·2 times, as high in 1891 as in the average for the decade 1851–60.

Table A.—Mortality from Cancer in England and Wales per Million living at all ages.

	1851–60.	1861–70.	1871–80.	1881.	1882.	1883.	1884.
Males	195	244	315	364	364	381	405
Females ...	434	523	622	668	692	702	707
Persons ...	317	387	473	520	534	549	563
	1885.	**1886.**	**1887.**	**1888.**	**1889.**	**1890.**	**1891.**
Males	411	424	456	450	488	512	518
Females ...	713	733	748	761	790	830	855
Persons ...	572	572	615	621	656	676	692

That cancer has really increased in this country appears to be now generally assumed in medical circles.

Sir Spencer Wells, in his Morton Lecture (November, 1888), gives the Registrar-General's figures, showing that the mortality from cancer had increased from 488 per million of the population in 1877 to 615 per million in 1887 in England and Wales, and from 350 per million in 1877 to 430 per million in 1887 in Ireland, as proof of this increase; and further quotes the statement of Dr. Fordyce Barker, that the mortality from cancer in the city of New York had risen from 400 per million in 1875 to 530 per million in 1885.

The attitude taken by the Registrar-General's Department has become somewhat modified of late years, doubts having been formerly

cast on the reality of the increase, whereas now the generally received medical opinion appears to have been adopted. Thus, Dr. Ogle (p. xiv, Supplement to the 45th Annual Report (1882) of the Registrar-General) points out that the deaths ascribed to *malignant disease* in the decennium 1851-60 amounted to 317 annually per million persons living; in the next decennium it had risen to 387; and in the decennium 1871–80 to 473; but adds: "there can be very little doubt that a considerable part in this apparent increase is simply due to improved diagnosis and more careful statement of cause on the part of the medical men."

In the 46th Annual Report (1883) of the Registrar-General (p. xviii), are the following remarks, bearing on the same point: "How much, if any, of this increase (in cancer) was real cannot be stated with any accuracy; but that some part of the apparent increase was only apparent, and due to improved diagnosis and more careful statement of cause, can, as we stated in previous reports, scarcely be doubted. Year by year the number of deaths ascribed to "abdominal disease" and other imperfectly stated causes has been undergoing diminution, and there has been, of course, a corresponding addition to the mortality under the more definite headings. Moreover, the increase of mortality from cancer has been considerably greater in the male than in the female sex. Now, were the rise not only apparent but real, there would seem to be no reason why males should have suffered more than females; whereas the difference is readily intelligible on the hypothesis that the rise was, at any rate in great measure, really due to improved diagnosis. For the cancerous affections of males are in a much larger proportion internal or inaccessible than are those of females, and consequently are more difficult of recognition, so that any improvement in medical diagnosis would add more to the male than to the female figures."

In the 52nd Annual Report (1889) of the Registrar-General (p. xiii), attention is again drawn to the increasing mortality from cancer, which now amounted to 656 per million persons living, "showing a further increase upon the ever-growing rates previously recorded." Then follow these remarks (the italics are ours) :—

"Some of the increase is most certainly attributable to increased accuracy in statement of cause, and to the system introduced some years back into this office of writing for further information in cases where some vague cause, such as 'tumour,' had been given as the cause of death in the original certificate; a system which added, for instance, in the year 1889 no less than 421 deaths to the heading 'Cancer.' Nevertheless, in face of the constant and great growth of mortality under this heading, and the expressed belief of medical practitioners specially engaged in dealing with this class of diseases that they are becoming more and more common, *it seems scarcely*

possible to maintain the optimistic view that the whole of the apparent increase can be thus explained; and it must be admitted, as at any rate highly probable, that a real increase is taking place in the frequency of these malignant affections."

It is evident, therefore, that although the view that the increase in cancer was chiefly, if not entirely, apparent has, until recently, been held in the English General Register Office, it is now reluctantly accepted as a probable fact that cancer has really increased; and it must be admitted that the figures on which this conclusion is based, as shown in the preceding table, look, at first sight, to be overwhelming in the weight of their evidence.

A more careful investigation, however, shows that the ratios prepared in the usual manner from the returns of the Registrar-General at the best only approximately represent the truth, and that, in fact, they may even be very misleading. Cancer is, *par excellence*, a disease of mature life. In a population of 1,000,000 adult males, aged from 25 to 35, about 95 would die annually from cancer; while there would be about 2,530 deaths among 1,000,000 males from 55 to 65, and 4,405 deaths in 1,000,000 males aged from 65 to 75. Therefore, to take the deaths from cancer at all ages in a community, and to compare them with the total population in order to arrive at the cancer death-rate, may introduce an error sufficiently serious to vitiate the results. If there be a larger proportion of lives, below, say, 50 years of age, the fraction formed by dividing the number of deaths from cancer by the total population will give an unduly small ratio; whereas, if the lives above 50 years of age be in excess, the ratio will be unduly large. Now the age distribution of one district may differ materially from that of another, and the age distribution of the males in a community may differ from that of the females, and the age distribution of the same district may possibly differ at different periods. That this consideration is of great importance is shown by Table I (Appendix), which gives the age distribution per million of population of each sex according to the census of 1881 in the several divisions of the United Kingdom.

It will be noticed that the average age of females in England and Scotland is higher than that of males, while the converse is the case in Ireland; and that the average age of the population in Ireland is much higher than in either England or Scotland. Consequently the death-rate from cancer given by the Registrar-Generals of the three divisions of the United Kingdom in their Annual Reports are unduly unfavourable to the female sex in Great Britain and to the male sex in Ireland; and similarly the death-rate from cancer in Ireland is exaggerated as compared with that of the sister kingdom.

To rectify this error it is necessary to assume a standard of age distribution, to be applied to each set of statistics examined. It is not

of importance what standard is selected, because, all the observations being treated alike, the comparisons instituted between them will be entirely trustworthy. It is desirable, however, to adopt a standard which is not purely arbitrary, and in the following investigations we have therefore used that given by the "English Life Table, No. 3, Persons," as the most suitable. This represents a stationary population unaffected by changes in the birth-rate or by migrations; and, although no existing community conforms to it, yet, for purposes of comparison of one community with another, it answers every purpose. The following is the age distribution according to this standard :—

Table B.—Age Distribution according to the English Life Table, No. 3, Persons.

Ages.	Population.
25—35	260,259
35—45	232,106
45—55	199,912
55—65	158,812
65—75	102,196
75 and upwards	46,715
	1,000,000

These considerations showing that a source of fallacy lurks in the rates of mortality usually quoted (especially when one country is compared with another), and that this fallacy specially affects the question of cancer, we came to the conclusion that it was desirable to investigate the alleged increase of cancer altogether afresh, and to avail ourselves in doing so of all the materials that could be turned to good account.

In order to be useful, the materials must extend over a long period of time, and must be presented in such form that, at any rate for several intervals of years, the deaths from cancer and the population may be grouped according to age and sex.

The records of life assurance offices of old standing might throw much light upon the subject, and doubtless if access could be had to them, and if the experience were collated in a suitable manner, an immense amount of most valuable information would be obtained. Unfortunately these sources, with one conspicuous exception, are not available. This exception is the Scottish Widows' Fund Life Assurance Society. For many years that institution has been accustomed at each septennial investigation to prepare a very complete statement of its mortality experience, distinguishing the deaths according to age and according to the causes of death, and at the same time giving the number of lives at risk in each interval of age. Through the

courtesy of the manager, Mr. A. H. Turnbull, these statistics have been placed at our disposal, and he has also kindly given us the figures for the four years 1888–91, which have elapsed since the last septennial valuation, thus enabling us to bring the investigation approximately up to date.

So far as we know, the British Empire Mutual is the only other office which has abstracted its experience in such a form as in the present connexion could be of any service. The experience of that Society, distinguishing diseases, was published for the two periods 1847–72 and 1872–78, and the following figures are extracted from the reports:—

Table C.—Experience of the British Empire Mutual Life Office.

Ages of lives.	Period 1847–72.		Period 1873–78.	
	Lives at risk.	Deaths from cancer.	Lives at risk.	Deaths from cancer.
25—35	42,448	1	13,151	1
35—45	61,136	8	18,397	3
45—55	43,887	18	19,332	6
55—65	17,410	12	12,825	15
65—75	3,973	4	4,225	9
75 and over	556	0	646	2
Total......	169,410	43	68,576	36

Unfortunately these facts are so scanty as not to afford trustworthy averages, and we have not seen our way to make further use of them. They show an apparent increase of about 50 per cent. in the death-rate from cancer in the period 1873–78 over 1847–72; but it must be remembered that, the office having been founded only in 1847, the lives assured had, in the earlier of the two periods, on the average much more recently passed the medical examination than in the later period. Cancer being a disease of comparatively slow development, this is a disturbing factor the effects of which it is impossible to measure. A life office must have been established for many years, and must for a long period have been transacting a business approximately uniform, before it is safe to base any conclusions upon its experience of such a disease as cancer.

The Scottish Amicable Life Assurance Society and the Clergy Mutual Life Office have each published their experience, giving the deaths from cancer and the lives at risk according to age; and in the collective experience of thirty American offices, published in 1881,

similar particulars are given; but in each of these cases only one period of time is investigated, and therefore, for present purposes, their figures cannot be utilised. On two occasions the Australian Mutual Provident Society, which is the largest life office in the British dominions, has investigated its mortality experience, and on each occasion has tabulated its cancer figures; but, for various reasons which we need not particularise, this information could not be successfully utilised.

The returns of the Scottish Widows' Fund, above mentioned, extend over four septennia and a broken period of four years, making a total period of thirty-two years during which it is possible to minutely investigate the causes and progressive rates of mortality. The figures, so far as they bear upon the present inquiry, are given in Table II in the Appendix.

The Scottish Widows' Fund statistics being only available in the form given in Table II, we proceeded to extract the Registrar-General's data for the same periods and in the same form, in order that trustworthy comparisons might be instituted; and we also treated in precisely the same way the valuable statistics of Frankfort-on-the-Main, to be described more minutely later on.

For England, Scotland, and Ireland respectively, the census enumerations of 1861, 1871, and 1881 were available for both males and females, the populations being classified in the same age periods as in the data supplied by the Scottish Widows' Fund. We had also the total populations, both male and female, enumerated in 1891, but information as to the age distribution of these populations had not hitherto been published. It was therefore necessary to assume that the age distribution in 1891 was the same as in 1881. A source of inaccuracy has been thus introduced, but its magnitude cannot be serious.* From the figures in the four census enumerations, the number of males and females respectively living in the middle of the years 1861, 1871, 1881, and 1891, were calculated for each age period 25—35, 35—45, 45—55, 55—65, 65—75, and 75 and over; and from these figures again the numbers living in each age group in the middle of each year from 1860 to 1891 inclusive were worked out. These were then classed into septennial periods, as with the Scottish Widows' Fund, with a broken period at the end. In this way was obtained with great accuracy the population in age groups in each division of the United Kingdom, passing through a year of life in each of the septennia under review. The deaths from cancer, arranged according to age, are given in the annual returns of the Registrar-General, and these were extracted and summed for the

* Owing to the steady fall in the birth-rate between 1881 and 1891, the average age of the population is probably somewhat higher at the latter date. This would tend to slightly exaggerate the apparent increase in the death-rate from cancer.

septennial periods. The annual returns for England and Wales and for Ireland came down to 1890 inclusive, but for Scotland the last year available was 1889, and the several observations were therefore closed at these points respectively. In the case of Ireland, we could not obtain the deaths from cancer prior to 1864, and we have therefore been limited to the three years 1864–66 for Ireland, instead of the seven years 1860–66.

For purpose of reference, and in case other inquirers desire further to investigate our figures, we give them in Tables II to V in the Appendix, arranged as above described.

From the figures in Tables II to V may be at once obtained the death-rates from cancer for each period of years, and for each age interval. As the resulting rates are necessary to the subsequent calculations, they are given in Tables VI to IX in detail, and are expressed as rates per million, in order to reduce the number of decimal places. The numbers, as might have been expected, run irregularly, but, on account of the method of grouping of the figures later on, this irregularity is not of any practical importance.

The rates of mortality given in Tables VI to IX, notwithstanding the considerable numbers on which they are based, do not run with sufficient regularity to disclose the general law by which they are governed; and, even though they did run regularly, it would not be easy to discover from them that law. Moreover, it is not the object of the present inquiry to ascertain the liability to cancer at different ages, but to discover whether cancer is on the increase or not in the community generally. It is therefore necessary so to group the figures that the total cancer experienced at all ages in any particular period of years may be compared with the total cancer experienced in any other period. If we take the rates of mortality given in Tables VI to IX and multiply them into the populations of Table B, we shall have the desired results. The sum of the products for any particular period of years will give the number of deaths from cancer per annum among 1,000,000 persons aged 25 and upwards. Then by comparing the sum for, say, the period 1860–66 with that for the period 1881–87, we can ascertain in which direction the apparent death-rate from cancer is progressing. It will be observed also that by pursuing this course the observations for all the different localities and all the different periods of years are reduced to one common standard, and the errors are eliminated which would arise from variations in the age distribution of the populations. Tables X and XI display these results in their final form, the ratios for ages under 55 and over 55 respectively, as well as for all ages, being given in them. Some persons may be glad to be able to investigate the matter for these two great periods of life, although we do not propose to include this branch of the subject in our inquiry.

In Tables X and XI the death-rates have been corrected for age distribution, and a single illustration will indicate how important is this correction, and how serious an error may result from its omission. Taking from Tables III and V the total population at risk for all ages, and the total deaths from cancer, and dividing the second by the first for each period of years for England and Ireland respectively, we shall have the death-rates from cancer in the form usually presented by the Registrar-General. They are given in the following table, and alongside them are placed the corrected rates from Table X.

Table D.—Comparison of Corrected and Uncorrected Cancer Death-rates.

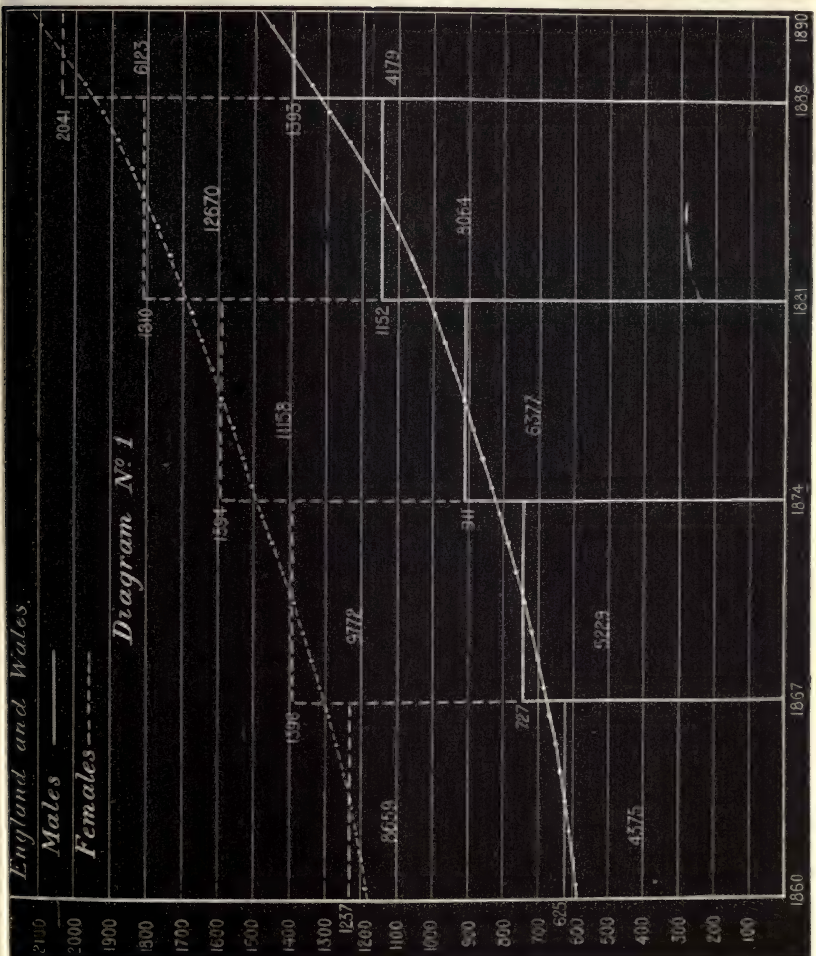
Period.	Not corrected.		Corrected.	
	England.	Ireland.	England.	Ireland.
1860–66	498	553	625	614
1867–73	597	627	747	661
1874–80	719	680	911	699
1881–87	902	807	1,152	824
1888–90	1,091	894	1,393	912

It will be observed that by the uncorrected figures Ireland stands a little above England for the first two periods, and a little below it for the other three, but that no very great difference appears between the rates for the two countries. The corrected figures, however, show that Ireland stands below England throughout, so that in the first two periods the position of the countries is reversed by the correction, and in the last three periods the difference in favour of Ireland is very great indeed. It is evident that the ordinary method of presenting the statistics exaggerates the rate of cancer in Ireland as compared with England, a result which, as already explained, might have been expected, owing to the age distribution of the populations of the two countries.

Much light may be thrown on the subject by a careful analysis of Tables X and XI. It is, however, difficult from arrays of figures to ascertain their exact teaching, and it is, therefore, desirable to aid the mind by translating the figures into a graphic form. In Tables X and XI the rates of mortality from cancer are given for five periods of years, and from them the rates may be obtained for each individual year. If these subdivided results be then plotted out in curves, the forms and directions of the curves will show at a glance, far more conclusively than could the most elaborate examination of

tabulated figures, the nature of the progression of the rates of mortality.

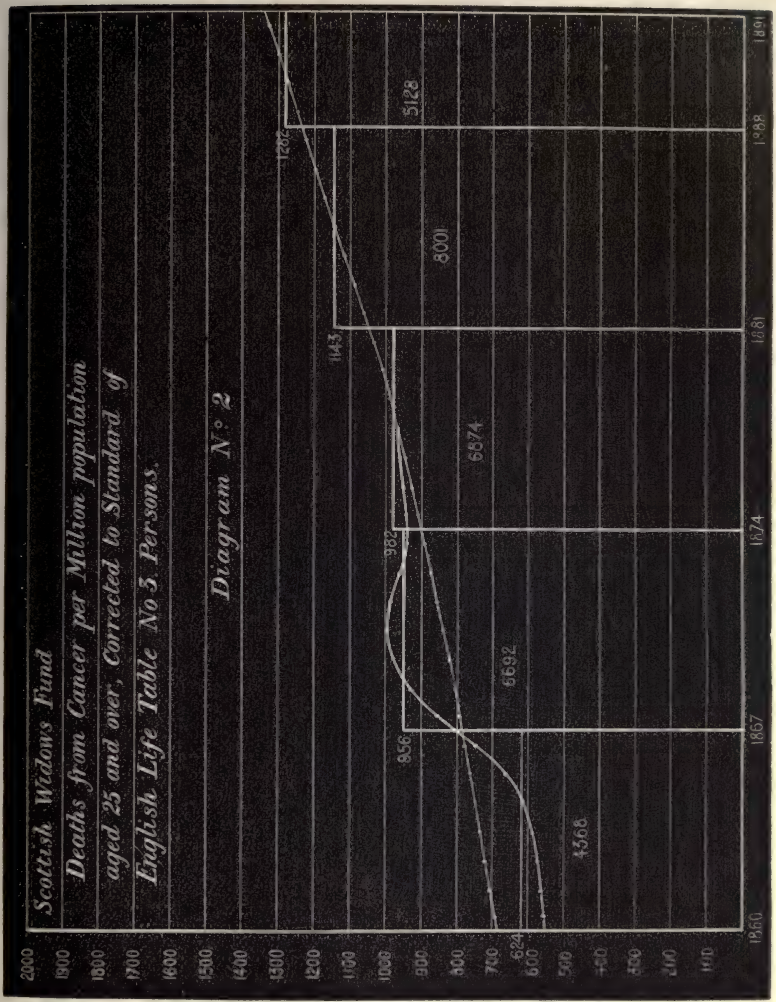
This distribution into individual years might be accomplished by skilful application of analytical processes; but the method would be very difficult on account of the nature of the material available, and the results could scarcely fail to be unsatisfactory and untrustworthy. The end can be much more satisfactorily attained by a modification of the late Joshua Milne's graphical method of constructing mortality tables, which has been fully described, and of which the beautiful accuracy has been demonstrated and illustrated by one of us (G. K.) in two papers in the 'Journal of the Institute



of Actuaries,' vol. xxiv and vol. xxx. As showing the application of the method to the present inquiry, the accompanying diagram, No. 1, relating to England and Wales, is submitted, of which a very few words of explanation will suffice. Along the abscissa axis are marked off equal lengths to represent each of the periods of seven years under review, with a portion of proportionate length for the three years 1888–90; and along the ordinate axis the rates of mortality per million are marked off. Rectangles are then erected, the areas of which are to represent the number of deaths from cancer in each of the septennial periods. Thus the area of the rectangle for the septennium 1860–66 is 4,375 for males, as its base is 7 and its altitude 625. Similarly for the other rectangles.

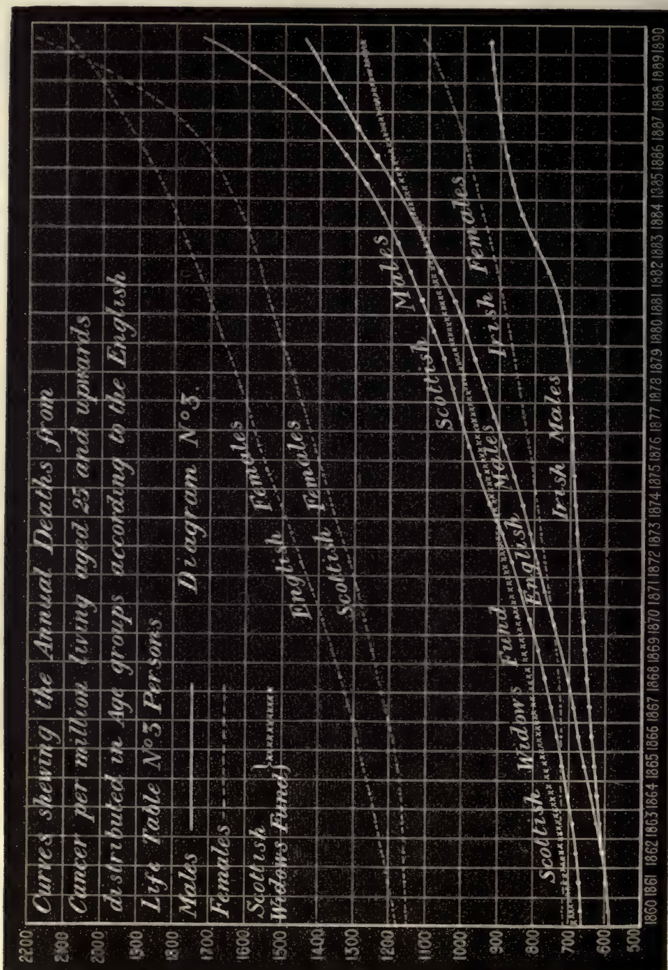
Through the tops of the rectangles must then be drawn a continuous curve in such a way that the area cut off is exactly equal to the area added. The length of the ordinate of the curve which is central to any particular year will then give the deaths from cancer in that year; and the accuracy of the drawing of the curve will be proved, if there be no sudden change of direction, and if the sum of the numbers for the seven years of a septennium is equal to the area of the rectangle for that septennium. Diagram No. 1 shows the curves for England and Wales, that for males being in an unbroken line, and that for females in a dotted line. Similar curves were prepared for all the observations so far discussed, and they are collected in Diagram No. 3, so that they may be easily compared. The curves for males are in continuous lines, while those for females are broken, and that for the Scottish Widows' Fund is marked by crosses.

The only curve which proved at all intractable was that for the Scottish Widows' Fund in its earlier portion. It will be seen from Table X that the death-rate from cancer in the first septennium was very light, while in the second it was comparatively heavy, very nearly equal to that in the third, and that the rates in the third, fourth, and fifth exhibit an almost uniform upward progression. These peculiarities are better seen by the help of Diagram No. 2, where the deaths are represented by the rectangles. The second and third rectangles being of nearly the same altitude, while the first is much lower, and the fourth considerably higher, there is indication of a rapid rise in the curve between the first and second periods and of a slight fall between the second and the third, and effect has been given to this in the undulating curve shown in the diagram. The most unpractised eye will, however, at once perceive that the undulating line cannot represent the real law of the curve, and more especially so when the other six curves are examined (Diagram 3). Except for the one bend in the Scottish Widows' Fund curve, they all partake of one character, and the conclusion is forced upon us



that the twist in the Scottish Widows' Fund curve is abnormal, and that it must be removed if we are to arrive at the true teaching of the facts. The data are given in septennial periods, and the shape of the figure suggests that were it possible to rearrange them into, say, quinquennial periods, the abnormality would disappear. The abnormality is introduced by insisting on the rule that the area of each portion of the curve must be exactly equal to the area of its corresponding rectangle, a rule which is to a certain extent arbitrary. Where, as in the case of the Scottish Widows' Fund experience, the original facts are few, and insufficient when grouped into short

periods to form trustworthy averages, it is better to enlarge the rule, and to redistribute the deaths slightly as between the adjacent periods, while still arranging that the curve shall in general outline follow the summits of the rectangles, and that the area of the curve shall exactly equal the areas of the rectangles in the aggregate. In Diagram No. 2 the correction of the curve on these principles is shown, and it is this corrected curve which is repeated in Diagram No. 3. How slight the correction really is becomes apparent when



we state that it is equivalent to the transfer of only seven deaths in the Scottish Widows' Society from the second to the first septennium,

and of one death from the third to the second septennium. Where the data are so scanty such slight adjustments are unavoidable. It may be added, however, that the general deductions to be drawn from the forms of the curves are the same whether we adopt the original or the corrected curve of the Scottish Widows' Fund.

In Table XII the distributed rates of mortality, as derived from the curves, are given for each of the seven sets of observations so far considered.

We are now in a position to examine more attentively the nature of the seven curves, and to attempt to derive from them the lessons which they are capable of conveying.

The *Irish* curves are the lowest of all, and are consistent with each other. In each of them there is a moderate upward gradient from the beginning to the end of the observations.

The *English* curves for males and females are far apart, the female mortality from cancer being very heavy. The two curves are, however, consistent with each other. They both show a decided and slightly increasing gradient from beginning to finish.

The *Scottish* curves are much nearer each other than the English, the males in Scotland having a higher apparent mortality from cancer than in England, and the females having one lower. The gradient in the Scottish curves is much the same as in the English, except that for the last year or two the rise is more rapid. The experience for the last period is for two years only in Scotland, while for England and Ireland it extends over three years, and this may account for the difference. Probably for this reason the curves at the end are more to be depended on for England and Ireland than for Scotland.

The correction for age distribution which we have introduced brings out the fact that Ireland seems to enjoy a great comparative immunity from cancer. Probably deficient accuracy in diagnosis and certification may account for much, if not all, of this difference. Ireland is a poor country, the majority of whose inhabitants cannot afford to pay much for medical attendance. The resulting deficient medical attendance would tend to produce defective diagnosis, and thus to lower the cancer curve. Probably also, owing to the poverty of the patients and consequently of the medical attendants, the average skill of the general practitioners over large tracts in Ireland is less than in Scotland and England, and this again would lead to defective diagnosis.

It is probable that in Scotland the general practitioner has been in the past better educated than in England, and this would cause the curves for the sexes to approximate, because in the female, cancer being more commonly accessible in position, is more easily diagnosed

than in the male, and improvement in diagnosis would raise the curve for males in greater proportion than that for females.

This argument is, however, scarcely consistent with the fact that the curve for English females is the highest of the seven. It appears therefore probable that, apart from diagnosis, there is some cause in English female life more favourable to cancer than among Scottish women.

The *Scottish Widows' Fund curve* has the easiest gradient of all, and this seems strongly to point in the direction so frequently indicated by the Registrar-General, that the apparent increase of cancer in the community is due to increased accuracy in diagnosis and certification. The policy holders in the Scottish Widows' Office are insured on the average for substantial sums, and are, therefore, presumably well-to-do, and able to secure, on the whole, better medical attendance than the mass of the people. The diagnosis throughout has, therefore, probably been good, as suggested by the fact that the curve begins comparatively high. But even among the most highly skilled members of the medical profession diagnosis has been improving, and therefore the Scottish Widows' Fund curve rises like the others. Among the class of practitioners attending assured lives there is not, however, the same scope for improvement in diagnosis and certification of death as in the profession as a whole, and the gradient of the Scottish Widows' Fund curve is consequently easy. This curve might have been expected to be below those for English and Scottish males, because, although there is a small admixture of females who suffer more severely from cancer than males, yet the lives are select. Persons with marked cancerous family histories are excluded. Cancer, moreover, is a disease whose development is usually gradual and slow, and may be preceded for several years by non-malignant disease of the part subsequently affected with cancer; hence, for two, or even three or four, years after an initial medical examination, cancer among insured lives should be comparatively rare. Notwithstanding these considerations, the Scottish Widows' Fund curve is above all the other curves for males at the commencement, though it falls below those for England and Scotland at the finish. This, as we have already remarked, points to good diagnosis on the part of the medical attendants of the assured lives, and to an improvement in diagnosis on the part of the medical profession generally during the last thirty years.

Another reason for thinking that the apparent increase in cancer is at any rate mostly due to improved diagnosis is derived from a comparison of the curves for males and females respectively. It will be noticed that the curves for females are always the higher, and that in each pair of curves the difference is practically constant through-

out the entire period. Now, if there were a real increase of cancer, there is no sufficient ground for thinking that this would be confined to any one set of organs of the body, or would affect one sex more than another; and in such case the difference between the cancer in males and females would be a percentage of the total, and would increase at the same rate as the curves themselves rise, and consequently the curves for males and females would tend to widen their distance apart. This, however, is not so. In each of the three pairs the curves for males and females do not diverge, but, if anything, tend to approximate.

It may be urged that, notwithstanding what has been said above, cancer may have increased more in certain parts of the body than in others, and that, although it has really increased in both sexes, it has increased in such greater proportion among males, that the curves for the two sexes remain parallel. This view, however, is contradicted by the Frankfort statistics, to be discussed presently, which confirm in a remarkable manner the conclusion we have drawn that it is only the cancer of organs common to both males and females which has apparently increased, while cancer of the special female organs, which is most easy of all to diagnose, has practically remained constant.

The chief weakness of the figures already given for cancer consists in (1) the absence of distinction between *carcinoma* and *sarcoma*, the two chief varieties of malignant disease, which are, however, pathologically distinct; and (2) the absence of statement of the part of the body primarily affected by the cancer.

An accurate statement of the site of the primary cancer in each case would enable us to ascertain whether the increase of cancer had been general, or chiefly in the cancer of inaccessible parts the diagnosis of which is comparatively difficult. Unfortunately medical certificates of death commonly omit any statement of the organ affected by cancer, and comparisons founded on those cases in which the position is stated in successive years are open to the fallacy that the non-localised cancers may have been transferred in increasing numbers to the more definite headings as time goes on.

There are no statistics available in which an accurate distinction is made between carcinoma and sarcoma, and the general terms "cancer" or "malignant disease" must therefore be regarded as including these two forms of malignant new growths in unknown proportions. It may be added, moreover, that no such statistics would be trustworthy unless each death were followed by an autopsy and by a microscopical examination of the diseased parts.

The town of Frankfort-on-the-Main is the only one known to us which has for a long series of years kept an accurate record of deaths from all causes, in which deaths from cancer are classified according

to the parts of the body primarily affected.* In the original the Frankfort figures are given in great detail, but we have summarised them somewhat. Thus, Table XIII shows both for males and females at all ages the total number of deaths from cancer of various parts of the body in the same year periods as we have adopted throughout this paper.

The subdivisions in Table XIII are still too minute, and the numbers consequently too small, for the purposes of useful investigation in the present connexion, and we therefore proceeded to group them, as shown in Table XIV, in three broad classes, which may be called "accessible cancer," comprising cancers of external parts of the body and other parts in which the nature of the disease is easily demonstrable during life by physical examination; "inaccessible cancer," comprising cancers of internal parts and other parts in which, as in the case of cancer of the bones, diagnosis is less easily made; and "cancer, position undefined," comprising simply the first line of Table XIII. In Table XIV only deaths at age 20 and over are included.

Under "Accessible cancer" we have included only the four headings Tongue, Mamma, Uterus, and Vagina, cancers of which are all capable of careful and exact diagnosis.

Under "Inaccessible cancer" come cancers primarily affecting any other parts of the body.

It will be seen from Table XIII that the great majority of cancers coming under this second head are of parts of the body in which the difficulties of diagnosis are great.

The classification cannot be regarded as perfect. Thus it may be pointed out that the first group embraces a large excess of women, among whom it is shown by the Registrar-General's returns in Great Britain that the apparent increase in cancer has been in less ratio than among men. In the next place, it may be argued that we have placed under the "inaccessible" division cancer of certain parts that might be more appropriately described as accessible. The following are the doubtful cases here referred to:—

* 'Statistiche Mittheilungen über den Civilstand der Stadt Frankfurt-am-Main' (1860—1889).

Table E.

Part affected.	1860—1866.	1867—1873.	1874—1880.	1881—1887.
Cancer of pharynx	..	..	1	1
„ œsophagus ..	8	8	18	39
„ rectum	21	28	39	32
„ prostate and bladder	8	10	6	16
„ penis	..	..	1	1
„ larynx and trachea	1	..	1	5
„ thyroid	1	..	..	..
„ bone	2	2	6	6
Total	41	48	72	100

Of the cancers enumerated above, cancer of the pharynx, thyroid gland, larynx, trachea, and penis are most accessible, but their number is so small (twelve in all) that their transference to the "accessible" group would have no appreciable effect; and since, before the paper took its present shape, they had been included with other cancers in their appropriate physiological system, and a considerable amount of calculation had been based upon that classification, they have been allowed to remain in the "inaccessible" group.

Cancer of the prostate and bladder may be regarded as intermediate between the "accessible" and "inaccessible" groups, but as cancer of these organs appears to have increased in a smaller proportion than the population, the effect of introducing it into the "inaccessible" group is rather to diminish the already striking difference which we shall show to exist between the increase in "accessible" and "inaccessible" cancer respectively.

Cancer of the œsophagus has, we think, been properly placed in the "inaccessible" group. The majority of cases of stricture of the œsophagus in persons over 50 are undoubtedly due to cancer, and yet certificates stating the cause of death at these ages as "stricture of œsophagus," without any definition of the cause of stricture, are still common. There can be no doubt that in the past they have been much more common, and that therefore cancer of the œsophagus is rightly placed in the class of "inaccessible" cancers which have been largely affected by improved diagnosis and certification of deaths.

Cancer of bone has also been placed in the same group. The number under this head is small, and, as the vertebræ and other inaccessible bones are a favourite seat of cancer of bone, the classification appears to us correct.

Cancer of the rectum is more accessible than that of other parts of

the intestines, and we have, therefore, extracted it separately, in order to ascertain whether its increase is in the same ratio as that of cancer of the upper parts of the intestine. The result is as follows:—

Table F.—Cancer of Rectum and other parts of Intestines in Septennial Periods.

Period.	Cancer of rectum.	Cancer of remaining parts of intestines.
1860–66	21	15
1867–73	28	19
1874–80	39	36
1881–87	32	47

Thus, while cancer of the rectum increased (in absolute amount and not relatively to the population*) 52·4 per cent., cancer of the rest of the intestines increased 213·3 per cent. It is evident, therefore, that by including cancer of the rectum in the group of "inaccessible" cancers we have further diminished the sharp contrast between "accessible" and "inaccessible" cancers.

In Table XIII we give the deaths from cancer in Frankfort during the period under review, grouped according to age and sex; and in the sub-headings, α, β and γ, in Table XIV, the same facts are classified as above described.

Census enumerations of the city of Frankfort have been taken at frequent but irregular intervals, and we have been able to avail ourselves of the returns for 1864, 1867, 1871, 1875, 1880, and 1885. From these we calculated the population in the middle of each year from 1860 to 1889 inclusive for both males and females, arranged according to age; and a summary of the results is given in Table XIV. On account of the military element which prevails on the Continent, there were some causes of disturbance, but, owing to the frequency of the censuses, these were not of much practical importance, and, moreover, they scarcely affected the ages which, in an inquiry into cancer, are principally concerned.

From the populations of Table XV and the deaths of Table XII the annual death-rates were calculated, corresponding to those in Tables VI—X, but it is scarcely necessary to reproduce them here. From these again were calculated the annual deaths from cancer in 1,000,000 living aged 25 and over, the population being as before

* The population of Frankfort increased from an average of 53,550 in the years 1860-66 to an average of 92,500 in the years 1881-87; *i.e.*, it nearly doubled. Therefore the *rate* of mortality from cancer of the rectum really diminished.

distributed in age groups, according to the English Life Table No. 3, Persons, as shown in Table B. A very slight adjustment was required in consequence of the Frankfort statistics being prepared for the decades 20—30, 30—40, &c., and not for 25—35, 35—45, &c., as at home. The final results are given in Table XVI.

The first thing that strikes one in examining this table is, the marked prevalence of cancer in Frankfort as compared with the United Kingdom, and we have failed to discover any facts in explanation of this, unless it be the extremely careful certification for which that city is noted.

Nevertheless the same general laws prevail as with all the other statistics available. Cancer preponderates greatly in the female sex, and, looking at the column relating to the total cancer, the progressive apparent increase for both sexes is observable. The Frankfort figures are, however, very instructive on account of the sub-division of which they are capable.

The cancer of undefined position does not show much sign of progressive change. No doubt on account of paucity of data, the numbers run irregularly, but from beginning to end of the thirty years under review there is no marked tendency to increase or decrease. This fact is of great importance, because, although in any one year there may be accidental fluctuations, yet, taken over such a long series of years, the figures become trustworthy. On the other hand, the rates of mortality from "inaccessible" cancer, both for males and females, steadily rise, though, perhaps, not quite so rapidly as in the United Kingdom. One very remarkable fact becomes apparent, namely, that males and females suffer almost equally from "inaccessible" cancer, the average for the thirty years being 1,641 for males and 1,640 for females. The excessive mortality from cancer of females is confined to "accessible" cancer, that is, practically to cancer of the female sexual organs.

The numbers relating to "accessible" cancer run somewhat irregularly, probably because of the paucity of data, among males there having been only 31 deaths in this category during the thirty years; but no well marked law of variation can be detected. There is, if anything, a tendency to decrease, at any rate as regards males, but the sequence of the numbers is not such that we could say with certainty whether or not that tendency would continue were the duration of the observations to be extended.

Taking a general view of the Frankfort figures, the one result of surpassing importance to be derived from them is that *in those parts of the body in which cancer is easily accessible and detected there has been no increase in the mortality from it between* 1860 *and* 1889.

It may be mentioned that in 1887 Dr. Grimshaw, the Registrar-General for Ireland, began to tabulate the deaths from cancer in

Ireland, according to the primary seat of the disease, whenever this is given in the death certificate. The number of years as yet available does not allow of any valid deductions being drawn, and the great variation during the four available years under the head of cancer of unspecified parts causes a further difficulty in utilising the figures.

In the 52nd Annual Report (1889) of the Registrar-General for England, the subject of the part of the body affected by cancer is also discussed, and in the words of the Registrar-General, "a sufficiently large sample of those cases of cancer in which the seat of disease is more or less clearly specified" is taken out to warrant the assumption that "such samples fairly represent the bulk" (p. 15). This method, however, does not appear to us to furnish trustworthy results, and we only refer to it here in passing.

To summarise, the conclusions arrived at from the whole investigation are as follows:—

1. Males and females suffer equally from cancer in those parts of the body common to man and woman, the greater prevalence of cancer among females being due entirely to cancer of the sexual organs, viz., the mamma, ovaries, uterus, and vagina. This is shown by the Frankfort statistics, and may not unreasonably be accepted as a probable general law, seeing that in other respects, where comparison is possible, the Frankfort statistics are confirmed by those of the United Kingdom.

2. The apparent increase in cancer is confined to what we have called "inaccessible" cancer. This is shown (*a*) by the Frankfort statistics; (*b*) by the fact that the difference between the rates for males and females respectively is approximately constant, and does not progressively increase with the apparent increase in cancer in each of the sexes; (*c*) because the apparent increase in cancer among the well-to-do assured lives, who are presumably attended by medical men of more than average skill, is not so great as among the general population.

3. The increase in cancer is only apparent and not real, and is due to improvement in diagnosis and more careful certification of the causes of death. This is shown by the fact that the whole of the increase has taken place in inaccessible cancer difficult of diagnosis, while accessible cancer easily diagnosed has remained practically stationary.

Table I.—Age Distribution of Population, 1881.

	Males.			Females.		
	England.	Scotland.	Ireland.	England.	Scotland.	Ireland.
25—35	329,343	331,560	267,966	323,414	308,369	276,664
35—45	256,363	251,086	238,453	249,403	244,905	243,902
45—55	186,820	186,487	193,671	186,897	189,069	188,308
55—65	130,641	128,238	155,272	133,194	136,285	157,686
65—75	70,493	71,981	95,822	75,865	82,514	88,429
75 and over	26,340	30,648	48,816	31,227	38,858	45,011
	1,000,000	1,000,000	1,000,000	1,000,000	1,000,000	1,000,000

Table II.—Statistics of the Scottish Widows' Fund Life Office.

Age.	1860—1866.		1867—1873.		1874—1880.		1881—1887.		1888—1891.	
	Population at risk.	Deaths from cancer.	Population at risk.	Deaths from cancer.	Population at risk.	Deaths from cancer.	Population at risk.	Deaths from cancer.	Population at risk.	Deaths from cancer.
25—35	12,921	..	26,299	3	40,102	2	48,224	2	32,298	2
35—45	19,611	6	29,368	6	46,986	10	64,820	12	43,401	16
45—55	19,665	12	24,671	18	33,418	16	45,424	32	30,393	32
55—65	12,305	12	16,641	34	20,964	41	25,397	69	17,019	35
65—75	4,732	11	7,160	25	10,208	41	12,345	40	8,273	28
75 & over	1,202	1	1,798	2	2,714	6	3,770	15	2,510	16
Total...	70,436	42	105,937	88	154,392	116	199,980	170	133,894	129

Note.—The "population at risk" in the above table is not identical with the "number of lives at risk" as shown in the Scottish Widows' Fund reports. The figures as given by the Scottish Widows' Fund produce the function known to actuaries as "rate of mortality," viz., the probability of dying in a year. A slight adjustment has therefore been introduced, so that the resulting function may be that known to actuaries as the "central death-rate" and called the "rate of mortality" by the Registrar-General. It is the ratio between the deaths in a year and the population in the middle of that year.

Table III.—Population and Deaths from Cancer. England and Wales.

Males.

Age.	1860—1866.		1867—1873.		1874—1880.		1881—1887.		1888—1890.	
	Population at risk.	Deaths from cancer.	Population at risk.	Deaths from cancer.	Population at risk.	Deaths from cancer.	Population at risk.	Deaths from cancer.	Population at risk.	Deaths from cancer.
25—35	10,065,013	603	11,026,867	729	12,120,353	840	13,209,827	983	5,967,814	566
35—45	8,127,262	1,604	8,599,671	1,862	9,416,398	2,326	10,283,966	2,953	4,615,998	1,551
45—55	6,096,059	3,132	6,654,352	4,073	7,044,307	5,108	7,480,991	7,147	3,379,692	3,951
55—65	4,067,022	4,512	4,441,718	6,032	4,835,955	8,088	5,237,894	11,174	2,366,329	5,988
65—75	2,204,804	3,936	2,450,509	5,273	2,636,088	7,184	2,824,396	9,999	1,275,981	5,621
75 & over	852,983	1,855	917,480	2,395	983,176	3,084	1,055,489	3,921	476,838	2,095
Total ...	31,413,143	15,642	34,090,597	20,364	37,036,277	26,630	40,092,563	36,177	18,112,652	19,772

Females.

Age.	1860—1866.		1867—1873.		1874—1880.		1881—1887.		1888—1890.	
	Population at risk.	Deaths from cancer.	Population at risk.	Deaths from cancer.	Population at risk.	Deaths from cancer.	Population at risk.	Deaths from cancer.	Population at risk.	Deaths from cancer.
25—35	11,210,266	1,772	12,156,572	2,053	13,205,566	2,304	14,331,599	2,463	6,502,206	1,209
35—45	8,709,359	5,700	9,331,309	6,784	10,154,306	8,187	11,051,894	9,309	5,014,213	4,502
45—55	6,458,701	9,552	7,147,098	11,578	7,698,205	13,991	8,282,058	16,694	3,757,546	8,277
55—65	4,382,534	9,540	4,851,654	12,109	5,377,469	15,403	5,902,299	19,044	2,677,856	9,555
65 —75	2,562,017	6,881	2,835,339	8,747	3,095,421	11,304	3,361,830	14,753	1,525,252	7,945
75 & over	1,113,831	3,041	1,208,216	3,723	1,290,244	4,710	1,383,783	6,110	627,817	3,312
Total ...	34,436,708	36,486	37,530,188	44,994	40,821,211	55,899	44,313,463	68,373	20,104,890	34,800

Table IV.—Population and Deaths from Cancer. Scotland.

Males.

Age.	1860—1866.		1867—1873.		1874—1880.		1881—1887.		1888—1889.	
	Population at risk.	Deaths from cancer.	Population at risk.	Deaths from cancer.	Population at risk.	Deaths from cancer.	Population at risk.	Deaths from cancer.	Population at risk.	Deaths from cancer.
25—35	1,383,589	96	1,502,275	108	1,653,863	142	1,790,464	156	530,505	29
35—45	1,087,934	233	1,150,259	266	1,255,343	329	1,355,898	430	401,744	86
45—55	848,151	497	893,309	584	948,729	794	1,007,054	1,044	298,385	271
55—65	597,247	707	629,657	934	658,899	1,209	692,502	1,624	205,183	461
65—75	311,774	642	355,699	838	373,072	1,095	388,706	1,363	115,171	565
75 & over	136,116	326	143,341	423	154,400	550	165,505	723	49,044	450
Total...	4,364,811	2,501	4,674,540	3,153	5,044,306	4,119	5,400,129	5,340	1,600,032	1,862
Females.										
25—35	1,726,740	237	1,804,835	241	1,889,282	333	1,982,381	299	585,116	25
35—45	1,314,243	807	1,393,974	820	1,485,077	1,008	1,574,353	1,199	464,699	206
45—55	1,006,538	1,328	1,070,748	1,611	1,144,345	1,869	1,215,463	2,153	358,753	552
55—65	745,462	1,445	785,837	1,669	830,411	2,071	876,125	2,491	258,595	809
65—75	427,081	1,147	480,346	1,388	507,408	1,668	530,467	2,106	156,566	870
75 & over	207,854	641	219,467	781	234,727	906	249,799	1,097	73,729	716
Total...	5,427,918	5,605	5,755,207	6,510	6,091,250	7,855	6,428,588	9,345	1,897,458	3,178

Table V.—Population and Deaths from Cancer. Ireland.

Males.

Age.	1864—1866.		1867—1873.		1874—1880.		1881—1887.		1888—1890.	
	Population at risk.	Deaths from cancer.	Population at risk.	Deaths from cancer.	Population at risk.	Deaths from cancer.	Population at risk.	Deaths from cancer.	Population at risk.	Deaths from cancer.
25—35	1,069,519	60	2,402,441	135	2,191,639	165	2,012,328	149	824,695	56
35—45	810,010	136	1,838,579	312	1,832,674	396	1,790,695	477	733,865	199
45—55	735,980	360	1,599,339	829	1,523,696	822	1,454,603	1,065	596,046	540
55—65	614,298	562	1,419,863	1,627	1,285,188	1,597	1,166,028	1,678	477,864	775
65—75	290,878	559	756,266	1,472	755,818	1,491	719,589	1,690	294,903	701
75 & over	132,009	342	327,464	853	357,385	935	366,700	1,001	150,292	480
Total...	3,652,694	2,019	8,343,952	5,228	7,946,400	5,406	7,509,943	6,060	3,077,665	2,751
					Females.					
25—35	1,171,915	101	2,709,747	286	2,494,890	302	2,284,409	261	930,913	112
35—45	909,481	308	2,019,910	771	2,039,629	836	2,013,894	923	820,678	416
45—55	812,882	708	1,785,658	1,496	1,664,419	1,510	1,554,855	1,744	633,616	869
55—65	667,354	731	1,533,696	1,867	1,416,704	1,981	1,302,003	2,076	530,576	952
65—75	310,299	573	773,450	1,405	767,154	1,519	730,158	1,608	297,545	720
75 & over	152,464	266	370,723	690	379,451	824	371,655	876	151,452	388
Total...	4,024,395	2,687	9,193,184	6,515	8,762,247	6,972	8,256,974	7,488	3,364,780	3,457

Table VI.—Annual Death-rate from Cancer per Million living at each Age Period. (See also Table II.)

Scottish Widows' Fund.

Ages.	1860—1866.	1867—1873.	1874—1880.	1881—1887.	1888—1891.
25—35	Nil	114·07	49·87	41·47	61·92
35—45	305·95	204·30	212·83	185·13	368·66
45—55	610·22	729·60	478·78	704·47	1052·87
55—65	975·21	2043·15	1955·73	2716·86	2056·53
65—75	2324·60	3491·62	4016·46	3240·18	3384·50
75 and over	831·95	1112·35	2210·76	3978·78	6374·50

Table VII.—Annual Death-rate from Cancer per Million living at each Age Period. (See also Table III.)

England and Wales.

Males.

Ages.	1860—1866.	1867—1873.	1874—1880.	1881—1887.	1888—1890.
25—35	59·91	66·11	69·31	74·41	94·84
35—45	197·36	216·52	247·02	287·15	333·84
45—55	513·78	612·08	725·13	955·36	1169·04
55—65	1109·41	1758·03	1672·47	2133·30	2530·50
65—75	1785·19	2151·80	2725·25	3540·23	4405·24
75 and over	2174·72	2610·41	3136·77	3714·87	4393·53

Females.

25—35	158·07	168·88	174·47	171·86	185·94
35—45	654·47	727·02	806·26	842·30	897·85
45—55	1478·94	1619·96	1817·44	2015·68	2202·77
55—65	2176·82	2495·85	2864·36	3226·54	3568·15
65—75	2685·78	3084·99	3651·85	4388·38	5208·98
75 and over	2730·22	3164·17	3650·47	4415·43	5275·42

Table VIII.—Annual Death-rate from Cancer per Million living at each Age Period. (See also Table IV.)

Scotland.

Males.

Ages.	1860—1866.	1867—1873.	1874—1880.	1881—1887.	1888—1889.
25—35	69·39	71·89	85·86	87·13	54·66
35—45	214·17	231·25	262·08	317·13	214·06
45—55	585·98	653·75	836·91	1036·69	908·22
55—65	1183·77	1483·35	1834·88	2345·12	2246·77
65—75	2059·18	2355·93	2935·09	3506·51	4905·75
75 and over	2395·02	2951·01	3562·18	4368·45	9175·42

Females.

Ages.	1860—1866.	1867—1873.	1874—1880.	1881—1887.	1888—1889.
25—35	137·25	133·53	176·26	150·83	42·73
35—45	614·04	588·25	678·76	761·58	443·30
45—55	1319·37	1504·56	1633·25	1771·34	1538·66
55—65	1938·40	2123·85	2493·95	2843·20	3128·44
65—75	2685·67	2889·58	3287·30	3970·09	5556·76
75 and over	3083·90	3558·65	3859·80	4391·53	9711·24

Table IX.—Annual Death-rate from Cancer per Million living at each Age Period. (See also Table V.)

Ireland.

Males.

Ages.	1864—1866.	1867—1873.	1874—1880.	1881—1887.	1888—1890.
25—35	56·10	56·19	75·29	74·04	67·90
35—45	167·90	169·70	216·08	266·38	271·17
45—55	489·14	518·34	539·48	732·16	905·97
55—65	914·87	1145·89	1242·62	1439·07	1621·80
65—75	1921·77	1946·41	1972·70	2348·56	2377·05
75 and over	2590·73	2604·87	2616·23	2729·75	3193·78

Females.

Ages.	1864—1866.	1867—1873.	1874—1880.	1881—1887.	1888—1890.
25—35	86·18	105·55	121·07	114·25	120·31
35—45	338·65	381·70	409·88	458·32	506·90
45—55	870·98	837·79	907·22	1121·65	1371·49
55—65	1095·37	1217·32	1398·32	1594·47	1794·28
65—75	1846·61	1816·54	1980·05	2202·26	2419·80
75 and over	1744·67	1861·23	2171·56	2357·03	2561·87

Table X.—Annual Deaths from Cancer in 1,000,000 living, aged 25 and over. Population distributed in age groups according to English Life Table No. 3, Persons (as shown in Table B).

Males.

		Under 55.	Over 55.	Total.
England and Wales.	1860–66	165	460	625
	1867–73	189	558	747
	1874–80	220	691	911
	1881–87	277	875	1152
	1888–90	336	1057	1393
Scotland	1860–66	185	510	695
	1867–73	204	615	819
	1874–80	250	757	1007
	1881–87	304	934	1238
	1888–89	246	1287	1533
Ireland	1864–66	152	462	614
	1867–73	158	503	661
	1874–80	178	521	699
	1881–87	227	597	824
	1888–90	262	650	912
Scottish Widows' Fund.*	1860–66	193	431	624
	1867–73	223	733	956
	1874–80	158	824	982
	1881–87	195	948	1143
	1888–91	312	970	1282

* The returns of the Scottish Widows' Fund include both males and females, and, owing to the form in which they have been prepared, it is not possible to discriminate the sexes. The proportion of females, however, is very small.

Table XI.—Annual Deaths from Cancer in 1,000,000 living, aged 25 and over. Population distributed in age groups according to English Life Table No. 3, Persons (as shown in Table B).

Females.

		Under 55.	Over 55.	Total.
England and Wales.	1860–66	489	748	1237
	1867–73	537	859	1396
	1874–80	595	999	1594
	1881–87	644	1166	1810
	1888–90	696	1345	2041
Scotland	1860–66	443	726	1169
	1867–73	472	798	1270
	1874–80	530	912	1442
	1881–87	570	1063	1633
	1888–89	422	1519	1941
Ireland	1864–66	275	445	720
	1867–73	285	466	751
	1874–80	307	525	832
	1881–87	360	588	948
	1888–90	423	652	1075

Table XII.—Deaths from Cancer per Million of persons aged 25 and upwards, distributed in age groups according to the English Life Table No. 3.

Year.	England and Wales.		Scotland.		Ireland.		Scottish Widows' Fund experience.
	Males.	Females.	Males.	Females.	Males.	Females.	Persons.
1860	587	1185	668	1155	591	717	695
61	597	1200	672	1157	599	718	708
62	608	1215	678	1160	607	719	723
63	619	1235	690	1163	614	720	740
64	637	1254	704	1169	622	721	756
65	655	1273	718	1181	629	722	763
66	672	1297	735	1198	636	723	783
67	685	1320	754	1212	643	727	800
68	705	1342	772	1226	649	733	816
69	725	1366	792	1247	655	740	830
1870	747	1394	815	1269	661	750	847
71	767	1421	840	1288	667	759	862
72	790	1450	865	1311	673	769	880
73	810	1479	895	1337	679	780	900
74	832	1507	924	1365	684	793	918
75	857	1537	957	1393	691	802	938
76	880	1565	980	1418	697	817	958
77	908	1594	1007	1440	699	832	972
78	940	1623	1036	1466	701	842	992
79	965	1652	1059	1492	703	860	1015
1880	995	1680	1086	1520	718	878	1038
81	1020	1705	1117	1542	737	891	1061
82	1058	1737	1147	1562	762	907	1085
83	1101	1775	1186	1589	800	935	1115
84	1143	1805	1224	1618	837	943	1143
85	1197	1840	1270	1657	860	965	1172
86	1245	1880	1329	1698	880	989	1200
87	1300	1928	1393	1765	892	1015	1225
88	1341	1985	1476	1870	902	1040	1250
89	1393	2038	1590	2012	912	1073	1270
1890	1445	2100	1740	2197	922	1112	1290

Table XIII.—Deaths from Cancer in Frankfort-on-Main at all Ages.

Part of body affected.	1860—1866.		1867—1873.		1874—1880.		1881—1887.		1888—1889.	
	Males.	Females.	Males.	Females.	Males.	Females.	Males.	Females.	Males.	Females.
Position undefined	33	29	18	30	42	20	58	55	19	17
Nervous system	5	3	8	5	4	1	10	10	..	..
Heart	..	1	..	..	..	..	..	..	..	..
Respiratory organs	2	3	2	1	8	4	8	3	1	2
Tongue	4	..	4	..	1	4	7	1	4	1
Œsophagus and pharynx	6	2	5	3	15	4	30	11	11	1
Stomach	36	55	46	58	80	73	104	121	34	29
Intestines	19	17	18	29	38	37	22	57	22	21
Intra- and retro-peritoneal	5	5	6	16	9	18	9	20	1	6
Liver	23	27	36	40	51	57	67	72	14	36
Pancreas and spleen	..	1	1	1	4	1	1	4	..	1
Kidneys	1	1	4	2	4	2	7	3	3	..
Prostate, urinary bladder, and penis	7	1	10	4	7	1	14	5	3	4
Uterus	..	69	..	96	..	117	..	159	..	64
Ovaries	..	9	..	9	..	17	..	20	..	12
Mamma	..	43	..	42	..	60	..	41	..	21
Vagina	..	1	..	2	..	5	..	5	..	3
Bone	2	..	2	..	5	1	5	1	..	..
Total	143	267	160	338	268	422	342	588	112	218

Table XIV.—Deaths from Cancer in Frankfort-on-Main over 20 Years of Age.

Age.	1860—1866.		1867—1873.		1874—1880.		1881—1887.		1888—1889.	
	Males.	Females.	Males.	Females.	Males.	Females.	Males.	Females.	Males.	Females.
20—30	3	5	5	3	7	5	6	15	1	3
30—40	10	13	17	32	19	40	34	50	4	20
40—50	18	51	20	65	47	92	57	123	26	41
50—60	50	86	40	95	60	112	94	143	39	61
60—70	36	70	47	80	79	99	88	151	24	61
70—80	19	35	26	54	43	67	46	75	16	24
80 and over	4	4	3	5	5	5	5	20	2	6
Total....	140	264	158	334	260	420	330	577	112	216

Table XIV—*continued*.—Deaths from Cancer in Frankfort-on-Main over 20 Years of Age.

Age.	1860—1866.		1867—1873.		1874—1880.		1881—1887.		1888—1889.	
	Males.	Females.	Males.	Females.	Males.	Females.	Males.	Females.	Males.	Females.
α. Accessible Cancer.										
20—30	0	1	0	0	0	0	0	4	0	0
30—40	0	6	1	19	0	21	1	26	0	12
40—50	0	36	2	28	0	53	4	63	2	23
50—60	6	40	0	51	0	51	1	62	0	27
60—70	3	25	6	30	0	46	0	34	2	21
70—80	0	7	0	14	1	14	1	13	0	5
80 and over	1	0	0	1	0	1	0	4	0	1
Total....	10	115	9	143	1	186	7	206	4	89
β. Inaccessible Cancer.										
20—30	1	4	3	3	6	5	1	10	0	3
30—40	4	6	13	11	17	15	21	21	4	5
40—50	14	9	15	33	40	36	44	46	18	14
50—60	34	36	35	34	50	54	80	64	32	32
60—70	27	39	38	44	71	50	78	104	20	37
70—80	16	23	25	36	32	50	42	59	13	15
80 and over	1	4	3	3	4	4	3	14	2	5
Total....	97	121	132	164	220	214	269	318	89	111
γ. Cancer, Position Undefined.										
20—30	2	0	2	0	1	0	5	1	1	0
30—40	6	1	3	2	2	4	12	3	0	3
40—50	4	6	3	4	7	3	9	14	6	4
50—60	10	10	5	10	10	7	13	17	7	2
60—70	6	6	3	6	8	3	10	13	2	3
70—80	3	5	1	4	10	3	3	3	3	4
80 and over	2	0	0	1	1	0	2	2	0	0
Total....	33	28	17	27	39	20	54	53	19	16

Table XV.—Population at Risk in Frankfort-on-Main.

Males.

Age.	1860—1866.	1867—1873.	1874—1880.	1881—1887.	1888—1889.
20—30	90,404	76,174	90,222	96,183	28,630
30—40	42,294	49,166	69,410	87,654	27,072
40—50	29,201	30,964	40,993	57,471	19,306
50—60	18,924	20,036	23,860	30,224	10,102
60—70	10,850	11,556	12,918	16,158	5,224
70—80	3,700	4,228	5,116	6,292	1,973
80 and over	617	750	919	1,137	366
Total ...	195,990	192,874	243,438	295,119	92,673

Females.

Age.	1860—1866.	1867—1873.	1874—1880.	1881—1887.	1888—1889.
20—30	74,536	85,365	108,009	133,871	41,545
30—40	40,880	50,361	70,137	91,459	28,576
40—50	27,260	31,019	41,705	60,116	20,364
50—60	19,645	21,320	26,813	34,575	11,467
60—70	12,172	13,881	16,524	21,439	6,926
70—80	4,272	5,288	7,008	8,957	2,786
80 and over	1,110	944	1,077	1,904	732
Total ...	179,875	208,178	271,273	352,321	112,396

Table XVI.—Frankfort.

Annual Deaths from Cancer in 1,000,000 living, aged 25 years and over. Population distributed in age groups according to English Life Table No. 3, Persons, as shown in Table C.

Males.

	Accessible.	Inaccessible.	Position undefined.	Total.
1860–66	126	1118	359	1603
1867–73	88	1421	137	1646
1874–80	14	1913	363	2290
1881–87	35	1865	305	2205
1888–89	74	1888	356	2318

Females.

	Accessible.	Inaccessible.	Position undefined.	Total.
1860–66	1081	1323	293	2697
1867–73	1214	1540	254	3008
1874–80	1220	1588	131	2939
1881–87	981	1820	272	3073
1888–89	1329	1930	256	3515

"An Experimental Investigation of the Nerve Roots which enter into the formation of the Lumbo-sacral Plexus of *Macacus rhesus*." By J. S. Risien Russell, M.B., M.R.C.P., Assistant Physician to the Metropolitan Hospital. Communicated by Professor V. Horsley, F.R.S. Received March 22,—Read May 18, 1893.*

(From the Pathological Laboratory, University College, London.)

CONTENTS.

Introduction.

The first part of my task is to express my great indebtedness to Professor Victor Horsley for enabling me to carry out this investigation under favourable circumstances at the Pathological Laboratory of University College, and for his great willingness at all times to criticise the results which I obtained.

In a paper on the functions of the nerve roots which enter into the formation of the brachial plexus of the dog,† I gave an account of the views that have been expressed and the work done in connexion with the limb plexuses. The hypotheses as to their significance advanced by Reil,‡ Scarpa,§ A. Monro,|| Sömmering,¶ and others were

* Part of the expenses connected with this research have been defrayed by a grant from the Scientific Grants Committee of the British Medical Association.

† 'Phil. Trans.,' 1892.

‡ 'De Nervorum Structurâ.'

§ 'De Gangliis et Plexibus.'

|| 'Observations on the Structure and Functions of the Nervous System.'

¶ 'Anatom.,' Pars quinta.

not alluded to, as they were mere conjectures, unsupported by any substantial evidence. The works of Krause, Schwalbe, Herringham, and Paterson were quoted as evidence of how far the problems connected with this subject have been elucidated by anatomical investigation, and the observations of Erb, Knie, and Thorburn alluded to as showing what advance had been made in the subject by the study of diseased processes in man.

Our knowledge of this subject has been greatly increased by numerous experimental researches, the latest of which is that by Sherrington. As his work is of such recent date and contains a historical account of all previous experimental researches that have been carried out in connexion with the lumbo-sacral plexus, it would be superfluous for me to do more than give a list of references to the various communications on the subject, including that of Sherrington, which list will be found at the end of this paper.

Anatomical Introduction.

The Lumbo-sacral Plexus in the Monkey.

Forgue, who does not mention what class of monkey he is dealing with, represents in diagrammatic form the nerves with the roots from which they are derived as follows:—The anterior crural from the 4th, 5th, and 6th lumbar nerve roots; the obturator from the same roots; and the sciatic from the 5th, 6th, and 7th lumbar and the 1st sacral nerve roots. The 2nd sacral root is also figured as sending a branch to the 1st sacral root before the latter enters the sciatic.

Sherrington describes two chief types of plexus in the monkey (*Macacus rhesus*); what he calls a "postfixed" and a "prefixed." The former is figured as having the following arrangement. The external cutaneous takes its origin from the 3rd and 4th lumbar; the 3rd lumbar also sending a branch to the 4th before the latter enters the anterior crural and obturator nerves, which it does in conjugation with the 5th lumbar root, both nerves also obtaining a filament from the 6th lumbar, while the sciatic is represented as springing from the 6th and 7th lumbar and 1st and 2nd sacral roots.

The representation of the "prefixed" plexus shows the origin of the external cutaneous nerve to be the same as in the "postfixed" type, while the branch from the 3rd to the 4th lumbar root is represented as joining the latter after it has given its branch to the external cutaneous nerve. This branch from the 3rd to the 4th lumbar apparently is supposed to send fibres to both the anterior crural and obturator nerves; these nerves also receiving branches from the 4th and 5th lumbar roots. The sciatic is figured as formed from the 5th, 6th, and 7th lumbar and 1st sacral nerve roots.

That Forgue's representation of the arrangement of the plexus does

not agree wholly with either of the types figured by Sherrington is evident; but further comparison of the descriptions of the two observers is rendered useless by the fact that Forgue omits to mention the class of monkey he made his observations on.

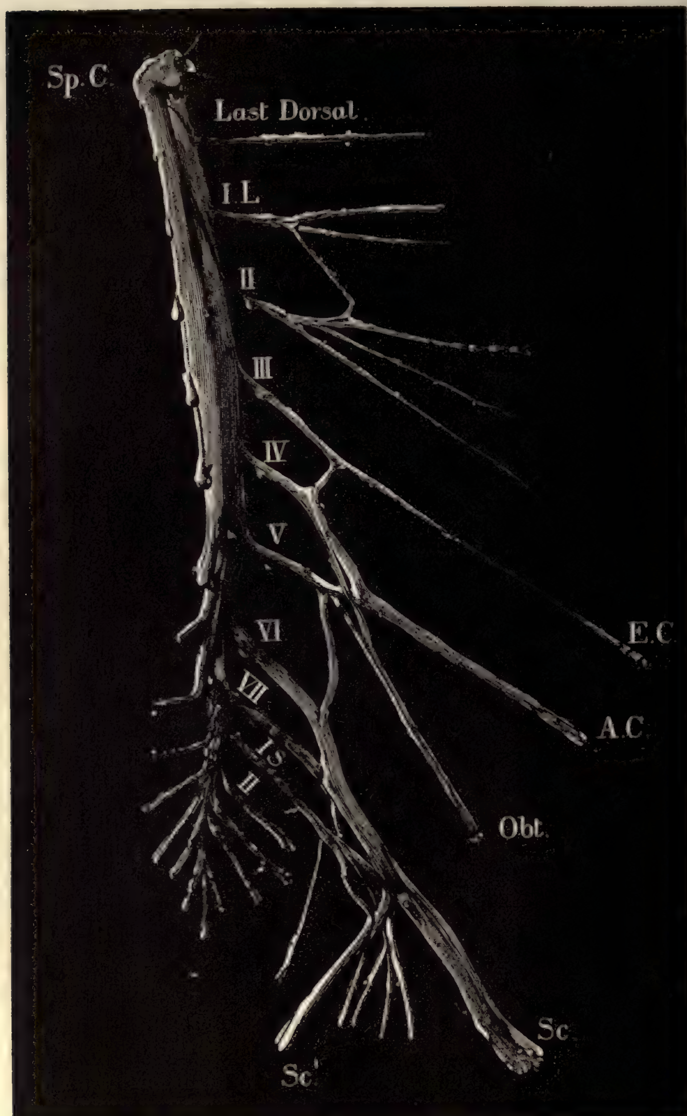
Like Sherrington, I confined myself to *Macacus rhesus* in my experiments on the lumbo-sacral plexus, and from numerous careful dissections, I am compelled to conclude that, at any rate in those animals dissected by me, one type occurred sufficiently frequently to make it necessary to look upon it as the chief one. But at the same time, among the different variations, one was conspicuously more frequent than any of the others, but scarcely sufficiently frequent, I think, to warrant my at present describing two main types, in the way that Sherrington has done.

The arrangement in what has been the most common type in the animals which came under my observation was as follows (see fig. 1):—The external cutaneous received fibres from the 3rd and 4th lumbar roots; the anterior crural from the 4th and 5th, as did the obturator nerve; and the 6th received a branch from the 5th lumbar root, before its junction with the 7th lumbar root to form the sciatic, which nerve also received a branch from the 1st sacral root. As far as I have seen, the 2nd sacral nerve root never sends a branch to the sciatic, in this type of plexus.

The variation which I have spoken of above as being the most common has the following arrangement (see figs. 2 and 3):—The external cutaneous is derived from the 3rd and 4th lumbar roots, as in the chief type; but the 3rd lumbar sends a branch to the 4th before the latter gives off its branches to the external cutaneous, the anterior crural, and the obturator nerves. The anterior crural then receives a branch from the 4th and another from the 5th lumbar roots, as in the chief type; but the obturator receives, in addition, a branch from the 6th lumbar root. The sciatic receives no branch from the 5th lumbar root, but only from the 6th and 7th lumbar and 1st sacral roots. I have been unable by the most minute dissections, aided by magnifying lenses, to trace any nerve fibres from the 2nd sacral root to the sciatic trunk. It will be thus seen that the chief points of difference between this and the most common type of plexus consist firstly in the absence of a branch from the 5th to the 6th lumbar root, and therefore to the sciatic; secondly, in the fact that the obturator nerve receives a branch from the 6th lumbar root in addition to those which it receives from the 4th and 5th lumbar; and, thirdly, in that the 3rd lumbar sends a branch to the 4th before the latter gives off any branches to the nerve trunks derived in part from it.

In the plexus of which fig. 2 is an example there can be no doubt that the 2nd sacral nerve root does not send a branch to the sciatic trunk, while that from which fig. 3 is taken shows how in some cases

FIG. 1.



it might easily be erroneously supposed to do so, and how, in such an arrangement, excitation currents might diffuse, with the greatest readiness, from the 2nd to the 1st sacral nerve root, and thus lead to fallacy.

FIG. 2.

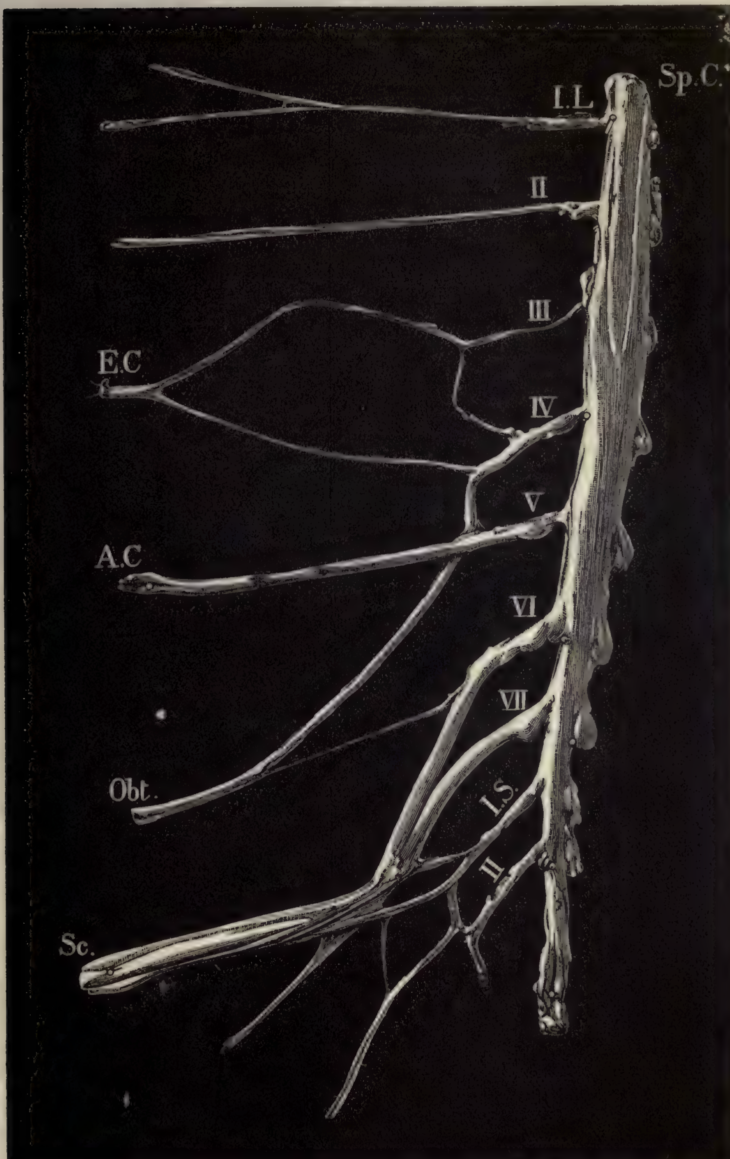
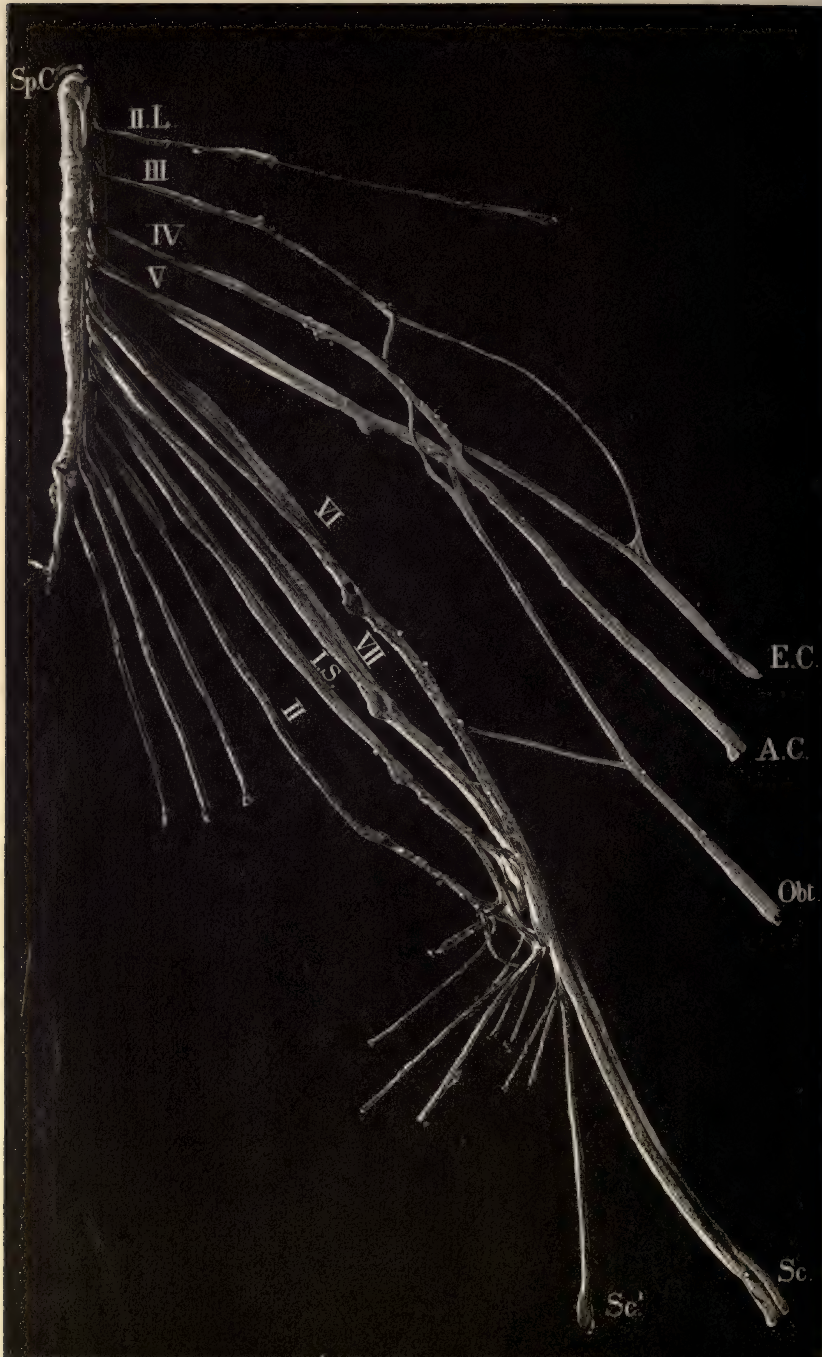


FIG. 3.



The similarity between the plexus which I have found most common and the "prefixed" plexus of Sherrington is obvious; but there is one point of difference. The branch figured by Sherrington as going from the 3rd to the 4th lumbar, and thus taking part in the formation of the anterior crural and obturator nerves, I have not found present. So, too, the variation which I have described as most common agrees with what he calls the "postfixed" type of plexus, with the exception that I have not found a branch from the 6th lumbar to the anterior crural; nor have I found that the 2nd sacral sends a branch to the sciatic. But the number of times I have met with this form of variation have not been sufficiently frequent to justify my denying the existence of such a branch as that from the 2nd sacral root to the sciatic. Indeed, its existence in some cases is rendered very probable by a comparison of this form of plexus with that most commonly met by me in the dog, which resembles it, and in which the 2nd sacral root sends a branch to the sciatic trunk. It is curious that in the species I have examined, the type of plexus most commonly met with in the dog should form the exception in the monkey, while that most commonly met with in the monkey should be the exception in the dog. The fundamental point of difference in the constitution of the two plexuses in the species I have examined has been the presence of a branch from the 5th to the 6th lumbar root in the one form of plexus, and its absence in the other, and it shows that the passage from the one type of plexus to the other is not an abrupt, but a gradual one, for, while in the majority of monkeys examined I found the branch from the 5th to the 6th lumbar root to be large, all graduations were met with down to the most minute filament connecting the two roots (see fig. 4).

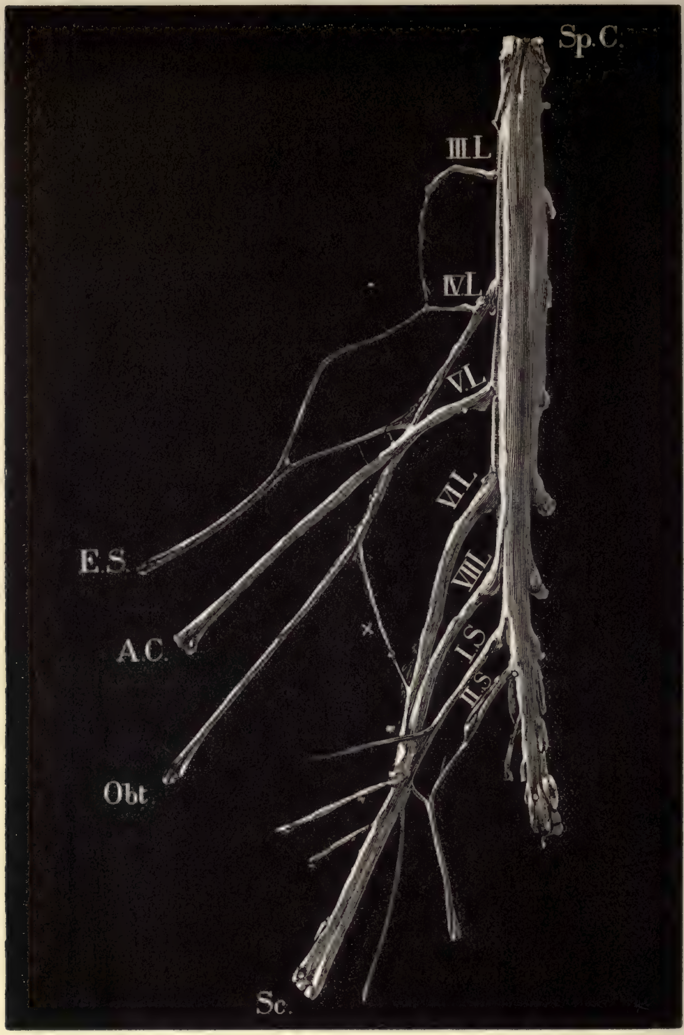
The Muscles of the Posterior Extremity of the Monkey, and their Actions.

In confirmation of Sherrington I find that the muscles of the posterior extremity of the monkey (*Macacus rhesus*) resemble very closely those met with in man; but a few points of difference exist, and must be briefly alluded to.

The gluteus maximus is a very thin flat muscle. There is no evidence of the existence of a peroneus tertius; but peroneo-tibialis muscle is sometimes present. The slip which the abductor hallucis in man occasionally sends to the first phalanx of the second digit is very large and constant, forming a separate muscle in fact.

As was explained in the paper on the brachial plexus of the dog, certain consequences of the action of the muscles deserve careful attention. In studying the mechanism by which the movements are brought about at the various joints, I found that a muscle might bring about a movement at a given joint upon which it has no direct

FIG. 4.



action, and I showed that this is due to the fact that certain muscles pass over more than one joint, and that they are not sufficiently long to allow of full movement taking place at one of those joints without their mechanically and passively pulling on their attachment on the other side of the other joint or joints. In the fore-limb this difficulty was chiefly encountered in connexion with the triceps, as when traction was made on this muscle, in addition to extension at the elbow joint, there resulted extension of all the lower segments of the

limb, owing to the arrangement of the muscles as has been explained above. So troublesome was this action when the movements at the wrist and digital joints were being studied, that an instrument was devised to exclude the action of the triceps under these circumstances. In the posterior extremity, the action of the quadratus femoris tends to produce, besides extension at the knee, extension at the ankle and of the digits. This action of the extensor muscle of the knee, though well marked in the dog, is much less so in the monkey, so that an assistant could fix the limb in such a position as to exclude this action without the aid of an instrument. It is obviously of great importance that this action of a muscle indirectly upon joints on which it has no direct action should be clearly recognised in all investigations of the movements at a given joint when any single nerve root is stimulated. If one muscle can directly or indirectly produce movements at so many joints, it is clear that we must first eliminate indirect effects of the action of this muscle before we can study the movements at these joints, as produced by the muscles which act directly on them, otherwise we should be led into error.

Operative Procedure.

a. Operation.—In every experiment the animal was narcotised by causing it to inhale ether; it was kept deeply under the influence of the anæsthetic during the whole of the experiment, and killed by an overdose of it at the end, except in those experiments in which the animals were allowed to live for some weeks for the study of the effect of section of nerve roots on its mode of progression and standing, and also to allow sufficient time for the degeneration in the peripheral nerves, consequent on such section of a nerve root, to develop. In these instances the operation, which was always a very trivial one, was done under strict antiseptic precautions, and the small wound afterwards dressed antiseptically. The animals were, of course, narcotised in these as in the other class of experiments. The neural canal was first opened, a ligature passed round the motor and sensory portions of a given nerve root together, as close to the spinal cord as possible, after which the two portions of the root were divided between the ligature and the cord. When the separate bundles of nerve fibres of which a nerve root is composed were under investigation, the whole root was dissected out to a varying distance beyond its point of exit from the neural canal, so as to allow of each separate bundle being separated from the others contained in a nerve root, for as long a distance as possible, in order to prevent diffusion of the current from one bundle of nerve fibres to another during the excitation experiments.

In those experiments in which the muscles were exposed by dis-

section, in order to allow of their being directly observed in action when a nerve root was excited, the spinal cord was always first divided transversely in the mid-dorsal region, so as to preclude all possibility of the animal's being conscious of any pain, a possibility most remote, seeing that it was always kept profoundly under the influence of the anæsthetic in this as in all other experiments on the nerve roots.

When intravenous injections of absinthe were given, a jugular or femoral vein was exposed, and a certain quantity of the essential oil injected into the vein selected, by means of a hypodermic syringe.

b. Excitation.—The distal portion of the divided motor root was separated from its sensory fellow, raised in the air by means of a ligature passed round it, and stimulated by means of fine platinum electrodes attached to the secondary coil of a du Bois Reymond's inductorium supplied by a bichromate cell. Exactly the same apparatus was used in excitation of the cortex cerebri (*vide infra*).

Division of Subject and Analysis of Results.

a. Division of the Subject.

As in the previous research, I approached the question by simple excitation of the peripheral end of the cut root, and observation of the compound movement thus produced forms the first part of the investigation. The next step was to carry out, if possible, a more minute analysis of this combined movement, dividing it into its component factors, *e.g.*, by using minimal currents of excitation, applied to the separate bundles of nerve fibres contained in the nerve root. The strength of current necessary to produce the maximum effect without diffusing to other nerve roots was found on the average to be 500 to 600 on Kronecker's inductorium scale. When minimum strengths of current were used for exciting the separate bundles of nerve fibres of which a nerve root is composed, the secondary coil stood at 40 to 50 on Kronecker's inductorium scale, on an average. But both these readings naturally varied considerably, according as the solution in the battery was fresh or old.

The way in which I was led to suppose that each separate natural bundle of nerve fibres of which a nerve root is composed might represent some simple movement, and that it might be possible to evoke such a simple movement by exciting these separate bundles of nerve fibres by minimal strengths of current, has been fully detailed in my former paper. This minute differentiation is not so easily carried out in the lumbo-sacral roots as was found possible in the cervico-dorsal roots of the dog, owing to the fact that the distance between the points of exit of the various nerve roots and those where they

combine to form the plexus is in most instances too small to allow of the minute bundles of nerve fibres being separated from each other for a sufficient length of their course. I was always able to differentiate several of the movements in each root, but was never able to differentiate all in any given root at the same operation. Another factor which contributes to make this process of minute differentiation so difficult is the delicacy of the nerve roots in the monkey as compared with those of the dog, as injury of the nerve fibres is more difficult to prevent in consequence, in the process of their separation from each other. With these difficulties to contend with, the process of differentiation of the various movements was aided by the division of muscles and tendons. That is, after it had been ascertained that a certain movement at a given joint resulted on excitation of a certain bundle of fibres, these muscles or their tendons were divided, and the same bundle of nerve fibres again excited in order to ascertain whether any other movement could be produced at the same joint, *e.g.*, after flexion at a joint had been produced, the flexor muscles acting on that joint were divided, in order to ascertain whether any fibres representing extension existed in the bundle of nerve fibres under investigation.

This minute differentiation forms therefore the second part of the investigation.

Thus far the investigation dealt with movements ; it was obviously necessary to pursue the matter further, and to see upon dissection what individual muscles were innervated by the various roots, or their parts when successively excited. This forms the third part of the work. As a corollary to this latter question, I have attempted to determine to what degree any given root supplies a muscle when the latter is innervated from more than one root, and whether any given muscle fibre is possibly supplied from more than one root.

The necessity of instituting new experiments in control of the results obtained by the foregoing methods led to the institution of the following procedures.

Of these, the first, constituting the fourth part of the investigation, consisted in dividing one or more roots, and then observing what effect was produced thereby in the direction of alteration of the natural gait or movement of the limb in progression and climbing.

Another control method, the results of which are embodied in the fifth part, was devised as follows :—A nerve root was divided (in some cases more than one), general epilepsy was then induced by intravenous injection of the essential oil of absinthe, and the resulting deficient participation in the fit of the limb in relation with the divided root or roots carefully observed.

As a corollary to this part of the subject, I tried whether the results obtained by the last method of experimentation are in any way altered when the root or roots are divided some weeks previously, instead of at the time when the epileptic spasms are induced.

*b. Analysis of Results.**

It was found most convenient and instructive to place together the results obtained in Parts I and II of the former investigation, and I see no reason for altering this method of description in detailing the following results.

Part I. Compound movements obtained by excitation of the whole nerve root.

Part. II. Minute differentiation obtained by excitation of the individual natural bundles of the nerve root.

1st Lumbar Root.—No intrinsic movement of the limb.

2nd Lumbar Root.—No intrinsic movement of the limb.

3rd Lumbar Root.—Slight flexion at the hip.

4th Lumbar Root.—Part I. Flexion at the hip, with the thigh adducted and the leg extended at the knee.

Part II. (1)†Flexion at the hip.
(2) Adduction of the thigh.
(3) Extension at the knee.

5th Lumbar Root.—Part I. Extension of the whole limb with adduction and internal rotation of the thigh and dorsiflexion of the foot.

Part II. (1) Adduction of the limb.
(2) Extension at the knee.
(3) Dorsiflexion of the foot.
(4) Extension of the digits.
(5) Extension of the hallux.

6th Lumbar Root.—Part I. The limb extended at the hip, adducted and rotated outwards; flexed at the knee, with the foot at right angles and everted at the ankle, the digits and hallux being flexed at the distal phalangeal joints.

* These results are based on a large number of experiments, and although variations were met with, the results which appeared to be the most constant are those given here.

† The small numbers in brackets denote the bundles from which a different movement was obtained when such bundles were separated in a nerve root. Only those bundles destined for the supply of muscles of the extremity are noted, which accounts in part for so few bundles being mentioned in connexion with some roots; but the variation in the size of the different roots is also responsible for this.

Part II. (1) Extension at the hip.
(2) Flexion at the knee.
(3) Dorsiflexion at the ankle.
(4) Extension at the ankle.
(5) Eversion of the foot.
(6) Extension of the digits.
(7) Flexion of the digits.
(8) Flexion of the hallux.
(9) Extension of the hallux.

7th Lumbar Root.—Part I. The limb extended at the hip, flexed at the knee, extended at the ankle, with the plantar surface of the foot looking inwards, the digits flexed at their metacarpo-phalangeal joints, and the hallux flexed and adducted into the sole of the foot, beneath the flexed digits.

Part II. (1) Extension at the hip.
(2) Flexion at the knee.
(3) Extension at the ankle.
(4) Flexion of the digits at their metacarpo-phalangeal joints.
(5) Flexion of the hallux.
(6) Adduction of the hallux.

1st Sacral Root.—Part I. Interosseal flexion of the digits, with flexion and adduction of the hallux.

Part II. (1) Flexion of the digits.
(2) Adduction of the hallux.
(3) Flexion of the hallux.

2nd Sacral Root.—No movement in the limb.

Part III. Direct Observation (after Dissection) of Muscles thrown into action by Excitation of the separate Nerve Roots.

I next attempted to ascertain as far as possible which muscles are thrown into action by stimulation of the several nerve roots. In exposing the muscles, great care was taken to separate them from each other, so that any communicated movement of one muscle to another should be avoided, as, unless this is done, it is sometimes difficult to be sure whether a particular muscle is contracting, or whether the movement observed in it is only communicated to it by an adjoining muscle which is in action.

Two plans were followed in this connexion. In one, each root was successively selected, and all the muscles in action on excitation of it were noted, while, in the other, a particular muscle, or group of muscles, was kept under observation while all the roots which contributed to the plexus were separately excited. Thus the results

obtained by the one method could be checked by those obtained by the other. In my former experiments I frequently was able to expose the muscles directly after the animal was killed, because the nerve roots, at the end of a prolonged experiment, retained their excitability for a considerable time, half an hour or more. In these experiments this plan has been abandoned, and exposure of the muscles during life alone relied on.

1st Lumbar Root.

No muscle related to the limb.

2nd Lumbar Root.

Psoas parvus.

3rd Lumbar Root.

Psoas parvus.
Psoas magnus.
Sartorius.

4th Lumbar Root.

Psoas magnus.
Iliacus.
Sartorius.
Adductor longus.
Quadriceps extensor.
Gracilis.

5th Lumbar Root.

Iliacus.
Gluteus minimus.
Adductor longus.
Adductor magnus.
Quadriceps extensor.
Gracilis.
Tibialis anticus.
Tibialis posticus.
Extensor longus digitorum.
Extensor proprius hallucis.
Tensor fasciæ femoris.

6th Lumbar Root.

Adductor magnus.
Hamstrings.
Soleus.
Gastrocnemius.
Tibialis anticus.
Tibialis posticus.
Extensor longus digitorum.

Flexor longus digitorum.
Peroneus longus.
Peroneus brevis.
Extensor longus hallucis.
Gluteus maximus.
Gluteus medius.
Gluteus minimus.
Pyriformis.
Obturator internus.
Obturator externus.
Gemellus superior.
Gemellus inferior.
Quadratus femoris
Popliteus.
Plantaris.

7th Lumbar Root.

Hamstrings.
Gastrocnemius.
Soleus.
Flexor longus digitorum.
Peroneus longus.
Peroneus brevis.
Flexor longus hallucis.
Adductor hallucis.
Interossei.
Gluteus maximus.
Gluteus medius.
Pyriformis.
Obturator internus.
Obturator externus.
Gemellus superior.
Gemellus inferior.
Quadratus femoris.
Popliteus.
Plantaris.

1st Sacral Root.

Intrinsic muscles of the foot.

Corollary to Part III.

In my former paper the question as to whether a single bundle of nerve fibres representing a single simple movement ever remains distinct in a nerve root during its course to the muscles it supplies without inosculating with other motor nerve fibres was considered,

and evidence in favour of an affirmative reply brought forward. The results which have been obtained in the present investigation have been in conformity with this view, so that I see no reason to alter the opinion formerly expressed.

A further point that was determined in this connexion was the question whether, when a muscle receives nerve fibres from more than one nerve root, both nerve roots supply nerve fibres to one and the same muscle fibre, and evidence was adduced to negative this possibility. The following evidence also negatives this view:—The peroneus longus muscle is supplied with nerve fibres from the 6th and 7th lumbar nerve roots, and the maximum contraction which can be evoked on excitation of the 6th root alone was greater than that evoked by stimulation of the 7th root alone; but neither effect was as great as when both roots were simultaneously excited. Another muscle chosen in order to test this point was the sartorius, which is supplied by the 3rd and 4th lumbar roots. When the 3rd root is excited with a minimal stimulus, the resulting contraction of the muscle is limited to its upper part, while similar excitation of the 4th root is followed by contraction of its lower part alone.

Part IV (Control). Alteration in the Action of the Posterior Extremity in Progression in Climbing or in Standing evoked by Section of a Nerve Root.

The following experiments were performed in order to observe the effect of division of one or more nerve roots on the movements of the limb during use in ordinary progression, climbing, &c. I observed the effects produced by division of a single nerve root, of two or three consecutive roots, and of two or three alternate roots on the same side. In no instance did I find that the division of a single nerve root was followed by any alteration in the movements of the limb, such as could be detected by running or climbing, twenty-four hours after the operation. A variable amount of paresis or paralysis of certain movements followed section of two or more nerve roots. Section of two consecutive nerve roots produced a greater effect than section of two alternate roots on the same side, an intermediate root being left intact. The effect in both these cases depended on the size of the roots divided, for even if the roots divided were two consecutive ones, and caused great impairment of any given movement, yet the effect noticeable would be much greater if the roots divided were large than if they were small, for the number of other movements weakened would be greater with large than with small roots. I have never found that the monkey's power of grasping the wire of its cage-house in climbing, after section of the 1st and 2nd sacral roots, has been impaired in such a manner as to be

detectable, but here again the experiment has not been repeated sufficiently frequently in the type of monkey in which, according to Sherrington, the 2nd sacral root supplies a branch to the sciatic nerve, which supplies the intrinsic muscles of the foot.

In no case did section of the 1st and 2nd lumbar roots cause any impairment in the movements of the limb, nor did section of the 2nd and 3rd in combination. When the 3rd and 4th lumbar roots were divided on the same side, flexion at the hip was greatly impaired. Movements at the knee appeared most affected when the 4th and 5th, or 6th and 7th, lumbar roots were divided together on the same side, roots which, as we have seen from the stimulation experiments, are concerned with the movements at the knee. The division of the last two roots mentioned also produced most effect on the movements at the ankle. The movements of the digits were most affected when the 7th lumbar and 1st sacral roots were divided together, roots excitation of which produced these movements. Section of the alternate roots, 3rd and 5th, 4th and 6th, 5th and 7th, 6th lumbar and 1st sacral, in combination, produced very little effect. In some cases it was possible to detect slight impairment of movements, but in others it was extremely difficult to be sure that there was any. If, however, three alternate roots, such as the 3rd, 5th, and 7th lumbar, or the 4th and 6th lumbar and 1st sacral, were divided together on the same side, there was no difficulty in detecting the general impairment of the movements of the limb.

In every case there was rapid improvement. In some this was so great that it was difficult to be certain that any impairment of movement remained, notably where alternate roots had been divided. In others, while most of the movements seemed as well performed as on the opposite side, impairment of a particular movement at a given joint remained, this being the case where two or more consecutive roots had been divided.

These results are in keeping with those which were obtained in connexion with the cervico-dorsal nerve roots of the dog. As in those experiments, reunion of the divided ends of the nerve roots was not the cause of the improvement in motor power, as there was not the slightest sign of any such reunion on post-mortem examination, and I have no other explanation to offer for the improvement in motor power which occurs, other than one of the hypotheses formerly advanced in explanation of the phenomenon. One of these suggested the possibility of a reflex inhibitory effect on the cells of the cortex cerebri by the section of a nerve root or roots, producing at first a greater degree of paralysis than would result from exclusion of the nerve root alone. The other supposed it possible that cortical impressions travelling down to the limb and meeting with a block, owing to the division of the fibres along which they formerly passed,

gradually become diverted, it may be through the anterior horn cells of the spinal cord, along other channels.

Part V (Control). Influence of Section of Root or Roots in excluding part of an Epileptic Spasm induced in the Limb by Intravenous Injection of Absinthe.

In this series of experiments an attempt was made to obtain further information as to the functional relations of the nerve roots to the muscles they supply, by the following method of experimentation. A nerve root was first exposed, but not divided; either a jugular or femoral vein was exposed, and 2 minims of the essential oil of absinthe injected into the vein by means of a hypodermic syringe. In order to evoke subsequent epileptic attacks in the same animal, doses of 1 minim of the oil of absinthe were found sufficient. In some cases, after several injections of absinthe, the excitability of the cortex became sufficiently increased to allow of general epilepsy being evoked by means of the induced current applied to the motor area of the cortex cerebri, a method which rarely succeeds in evoking general epilepsy in the monkey under other circumstances.

The first observation that was made in every instance was one to determine the position assumed by the limb during the general convulsions which followed the introduction of absinthe into a vein. In this way it was easy to exclude any error due to injury of any of the roots during the operation necessary to expose them. The position which the limb assumed when all the nerve roots were intact was one of flexion of thigh on the abdomen, with the leg at right angles to the thigh at the knee joint, the foot dorsiflexed at the ankle, and the digits flexed.

When the 3rd lumbar root was excluded, the position assumed by the limb was the same as on the opposite side on which all the roots were intact, with the exception of the flexion of the thigh on the abdomen being less pronounced.

Exclusion of the 3rd and 4th lumbar roots allowed extension at the hip to predominate over flexion, a result in keeping with the fact that excitation of both these roots produced flexion at this joint; while exclusion of the 5th lumbar root as well allowed flexion at the knee to predominate more markedly over extension. After section of the 6th lumbar on the same side, there was only feeble flexion at the knee, with extension instead of dorsiflexion of the root and flexion of the digits. The only intrinsic movement of the limb during general convulsions when the 7th lumbar was also divided was adduction and flexion of the hallux and flexion of the digits, movements which we have seen resulted on excitation of the 1st sacral root; and when

this root was also divided no intrinsic movement of the limb could be observed during the cortical discharge.

On excluding the 1st sacral root alone on one side, no tangible difference in the positions of the limbs on the two sides could be detected during the general convulsions evoked by absinthe.

Exclusion of the 7th lumbar alone caused an increase of flexion at the hip on that side, owing to the extensors being weakened, while there was more extension and less flexion at the knee, because the hamstrings are represented in this root, marked tibial dorsiflexion at the ankle, as the extensors were enfeebled, and flexion of the digits at the phalangeal joints. When the 1st sacral was divided in conjunction with the 7th lumbar root, the limb assumed exactly the same position as in the last experiment, except that the digits were not so powerfully flexed, and the hallux was not adducted, movements which excitation of these roots evoke. The marked feature which was noticeable after division of the 6th lumbar root, in addition to the two last mentioned, was that there was no sign of movement of the digits or hallux during the convulsions, because, of course, all the roots excitation of which produced movements of these parts were divided.

Combined section of the 5th, 6th, and 7th lumbar roots allowed of flexion at the hip, feeble extension at the knee, and flexion at the digits, with adduction at the hallux, during the cortical discharge. The foot remained quite motionless at the ankle joint, as was to be expected, seeing that excitation of no nerve root other than these produced movement at this joint.

Alternate roots were divided in the following combination, and the position of the limb observed during general convulsions. The 4th and 6th lumbar roots divided together caused flexion at the hip to be less marked, as was flexion at the knee, dorsiflexion at the foot, and flexion of the digits. There was thus a weakening of all the movements which predominated when all the roots were intact.

Section of the 5th and 7th lumbar roots, at the same time, was responsible for more marked flexion at the hip and knee and dorsiflexion of the foot. So that, although one of the roots which supplies the quadriceps extensor and one of those which supplies the hamstrings was divided, the section of that to the former group of muscles was attended with the greater result, for it allowed the remaining flexor root to predominate, so to speak, over the remaining extensor root to a greater extent than did the two flexors over the two extensors before any of the roots were divided.

Corollary to Part V.

It seemed desirable to test carefully the question as to whether section of a root or roots some time previous to that at which the

general convulsions were evoked is attended with the same results as when the section is done at the time when the convulsions are induced. I accordingly divided the 4th and 6th lumbar roots on one side, ten days before the day on which I proposed to excite the cortical discharge; and I divided the same roots on the opposite side at the time when the general convulsions were evoked. The result was that the positions of the two limbs were identical during the convulsions. The result of this experiment was confirmed by similar experiments with different combinations of nerve roots.

This method of experimentation was first employed by me in the investigation of the nerve roots which enter into the formation of the brachial plexus of the dog; and, as I have before pointed out, it serves the double purpose of being a means of checking the results of direct stimulation experiments, and of affording us the power of ascertaining whether elimination of a root does or does not result in incoordination of the remaining combination of movements. The results obtained by its use have abundantly confirmed those obtained by stimulating the individual nerve roots; and also prove that the coordination of the movement produced by the remaining roots is not in the slightest degree affected by the elimination of one or more of them. They also make it clear that there cannot be overflow of nerve impulses through the spinal centres, at any rate to any great extent, *i.e.*, impulses which should reach the muscle through the nerve root that has been divided do not under these circumstances reach them by other commissural channels.

Discussion of Results. Conclusions.

Stimulation Experiments.—A comparison of the results obtained by Ferrier and Yeo, those by Sherrington, and my own, shows that there is considerable difference of opinion as to which is the highest nerve root in the lumbar series excitation of which produces movement in the posterior extremity of the monkey. We are all agreed that flexion at the hip is the first movement of the limb evoked as we excite the lumbar roots from above downwards, but Ferrier and Yeo regarded the 4th lumbar as the first in the series from above down excitation of which caused this movement, whilst Sherrington states that in both types of plexus which he has described this movement was produced by excitation of the 2nd lumbar root, though it was feeble in the case of the "postfixed" class of plexus. In no instance have I observed this movement when the 2nd lumbar root was stimulated, the 3rd lumbar being the first root in the series from above down in which I have observed this movement to be represented.

Extension at the hip Ferrier and Yeo found represented in the 5th

lumbar root, while Sherrington and I have found the 6th lumbar root to be the first from above in which the movement is represented.

Extension at the knee Sherrington places as high as the 3rd lumbar root, while my own observations coincide with those of Ferrier and Yeo, who found the 4th lumbar to be the highest root in which this movement is represented.

We are all agreed that flexion of the knee is first represented in the 6th lumbar root from above, though Sherrington has also found it represented as high as the 5th lumbar root; rarely in the "prefixed" class of plexus, which resembles most the type of plexus I have most commonly met with.

Ferrier and Yeo do not mention dorsiflexion at the ankle as produced on excitation of any nerve root, while Sherrington and I are agreed that the 5th lumbar is the highest in the series in which this movement is represented.

We have all found extension at the ankle to be first represented in the 6th lumbar root from above; but, while Ferrier and Yeo and I find flexion of the hallux and digits first represented in the 6th lumbar root, Sherrington states that it is first represented in the 5th.

With regard to the inferior limit of supply to the limb, Ferrier and Yeo found this to correspond to the 1st sacral nerve root, and this has been the lowest root of the series from which I have been able to obtain any response in the limb.

Sherrington found that in the "prefixed" class of plexus which he has described, this is the lowest limit of root supply to the limb; but that in the "postfixed" class this limit extends as low as the 2nd sacral nerve root (9th post-thoracic root).

The number of times I have met with the type of plexus most resembling that described as "postfixed" by Sherrington has not been sufficiently frequent to justify my expressing any decided opinion as to the inferior limit of root supply to the limb in this class of plexus. All I can say is that I have never succeeded in evoking any intrinsic movement in the limb by excitation of the 2nd sacral root with currents of such strength as elicited movement when applied to other nerve roots, or even with currents very much stronger than this. Only by using such powerful currents that there could be no doubt as to the diffusion of the current beyond the root to which it was applied, was I ever able to elicit any intrinsic movement in the limb. Further, I have failed, by most careful minute dissections, aided by powerful lenses, to trace any nerve fibres from the 2nd sacral nerve root to the sciatic nerve. Of course the crucial test of this point would be to divide the 2nd sacral nerve root in this class of plexus, and observe whether degeneration in the sciatic nerve follows. This I have attempted, but unfortunately the animals in

whom I have divided the 2nd sacral nerve roots have, up to the present, had plexuses resembling the "prefixed" class. I have great difficulty in understanding how Sherrington finds any given muscle represented in so many more nerve roots, as a rule, than I do, and, conversely, how he finds so many more muscles, and, in consequence, movements, represented in certain roots. As an instance, the tibialis anticus is stated to be represented in the 5th, 6th, and 7th lumbar nerve roots, while I have only found it represented in the 5th and 6th lumbar roots. Then, again, the 1st sacral nerve root in the "prefixed" class of plexus, which corresponds most closely to the type of plexus I have most commonly met with, is said to produce extension at the hip with slight rotation outward of the thigh, flexion at the knee, extension at the ankle, strong flexion and abduction of the hallux and flexion of the digits in "interosseous" position; whereas, like Ferrier and Yeo, the only movements I have found most constantly represented in this root have been interosseal flexion of the digits, with flexion and adduction of the hallux.

The only way in which I can account for these very great differences in our results is by supposing that while I have only included those roots in which a given muscle is most commonly met with, and those movements, or muscles, most commonly met with in any given root, Sherrington has included every variation; for, as an example, all the movements mentioned by him as represented in the 1st sacral root, I have found represented in that root in rare instances, but never all represented together in any single animal. The movements most commonly met with in any single animal were those already mentioned, and when variation occurred it consisted in one or other of the other movements being added to these.

If I am correct in my interpretation of Sherrington's classification of results, I cannot help feeling that that which I have adopted is more instructive and less likely to lead to confusion.

With regard to the question whether the limb plexuses have an anatomical or physiological significance, I find it difficult to believe that the developmental processes which bring about these arrangements of nerve fibres do so on a purely anatomical basis without regard for physiological combination. Because excitation of a given nerve root with the induced current evokes a movement which may not resemble a natural one is to me no argument against the possibility that in this nerve root nerve fibres destined for the supply of certain groups of muscles are combined in such proportions as they are likely to be required in certain natural movements. The point is one which is exceedingly difficult to test by experiment, and those instituted by Sherrington with a view to solving this problem do not appear to me to be conclusive.

The facts that muscles or groups of muscles are represented in such

different degrees in different nerve roots, and that one group predominates in one root while another predominates in another, lend strong argument to the probability that the arrangement is in great measure a functional one. If the arrangement of nerve fibres in the nerve roots is a purely anatomical one, why should all the fibres destined for the supply of a given muscle not be contained in the same nerve root? What necessity would there be for the division of the fibres so that one set of them should be contained in one root, while another set is contained in another? These points are strongly opposed to the supposition that such an arrangement has been brought about without any regard for physiological action. Then, also, the fact that muscles which are known to act in consort are represented in the same nerve root is one which it is difficult to interpret by mere anatomical arrangement without regard for physiological laws.

Contrary to the observations of Sherrington, I find that the compound effect obtained on electrical excitation of a nerve root may be resolved into its component factors, when it is found that movements diametrically opposed to each other may be represented in the same nerve root, *e.g.*, flexion and extension.

It seems to me that some of Sherrington's own results point in this direction; for although he makes the statement that each small bundle of nerve fibres in a nerve root represents a miniature root, as it were, yet he finds that by using minimal currents differentiation was obtained in so far that one simple movement was elicited before another as the current was gradually increased in strength. The explanation I would offer for the different conclusions come to on this point by this observer and myself is that he excited the nerve fibres on the proximal side of the intervertebral foramina while I excited them on the distal side. The latter procedure makes it possible to separate the different bundles of nerve fibres contained in a nerve root for a greater distance of their course, and thus to avoid more effectually the possibility of diffusion of the current from the bundles of fibres actually excited to those juxtaposed. I am aware, from my own experience, that it is almost impossible to get any differentiations of movements in a nerve root unless the bundles of fibres of which it is composed be first traced well beyond the intervertebral foramen. That this should be the case is only natural, for what is more likely than that the fibres, packed together so closely as are the bundles of a nerve root in their passage through an intervertebral foramen, should make it very easy for the current applied to one set of fibres to diffuse to others in such close contiguity. That separation of one bundle of nerve fibres from another for a sufficient distance in their course is as important a factor in this differentiation of simple movements as is the use of minimum currents for excita-

tion in this connexion, is proved by the difficulties met with in obtaining differentiation in the case of the nerve roots which enter into the formation of the lumbo-sacral plexus, where the distance between the points of exit of the nerve roots from the neural canal and the point where they unite to form the plexus is not so great as in the case of the cervico-dorsal roots and the brachial plexus, and where, consequently, differentiation is not so easy to effect as in the latter case. If further proof were needed in support of the fact, which I have repeatedly convinced myself and others of, viz., that it is possible to separate the fibres concerned with one simple movement in a nerve root from those concerned with another simple movement, nothing could be more convincing than the results which I obtained with regard to the recurrent laryngeal nerve.* In this small nerve I found that it was possible to separate those nerve fibres concerned with abduction from those concerned with adduction of the vocal cords; so that electrical excitation of the one set of fibres evoked the one movement, while excitation of the other set evoked the other movement. If such differentiation be possible in a nerve of such small size, how much the more likely is it that it should be possible in a nerve root of so much greater proportions; unless it is argued that the structural arrangement of nerve fibres in a nerve root differs from that met with in a nerve trunk. My own observations leave no doubt in my mind that the structural arrangement in a nerve root is identical with that met with in a nerve trunk; and there is, besides, abundant proof of this from the observations of others.†

Such single simple movements bear an almost constant relation to the nerve roots, the same movements being, as a rule, found in any given root, and such movements always bear the same relation to the spinal level, *e.g.*, extension and flexion of the knee are represented together in one root, while extension is represented alone in the root immediately above this, and flexion is represented alone in the root immediately below this.

Each bundle of nerve fibres, representing a single simple movement in a nerve root, remains distinct in its course to the muscle or muscles producing such a movement, without inosculating with other motor nerve fibres. Additional evidence in support of this statement is supplied by the results obtained in the case of the recurrent laryngeal nerve, for in this nerve it was found possible to separate accurately the abductor from the adductor fibres, and to trace them by dissection to the abductor or adductor muscles of the larynx. And when one set of nerve fibres was divided while the other was left

* 'Roy. Soc. Proc.,' vol. 51, 1892.

† *Cf.* Herringham, Paterson, &c.

intact, degeneration resulted in the muscles of corresponding function, and in these alone: those of opposite function showing no sign of degeneration.

The group of muscles supplied by any given nerve root occupy both the anterior and posterior surfaces of the limb;* in other words, muscles whose unimpeded action would produce one movement are represented in the same root as others whose action would produce a movement diametrically opposite, *e.g.*, the flexor and extensor muscles of the ankle are represented in the same nerve root. In such combinations certain muscles are always more extensively represented than others; so that, with a current sufficiently strong to stimulate all the fibres in a nerve root equally, certain muscles predominate in their action over others. The 6th lumbar root contains fibres which supply the flexors of the digits and fibres which supply the extensors, and yet when the whole of the fibres in the nerve root are simultaneously and equally excited flexion of the digits is brought about owing to the flexor muscles predominating over the extensors. This predominance of one group over another does not always obtain, however, as in the case of the ankle joint, where both the flexors and extensors are supplied by nerve fibres derived from the 6th lumbar root; simultaneous and equal excitation of all the fibres contained in this root causes the foot to be fixed at right angles at the ankle, neither the flexors nor the extensors predominating, but the one set of muscles equalising the action of the opposite set.

When a certain group of muscles is found to predominate in its action in one root it, as a rule, predominates in that root, *e.g.*, I have not met with an instance in which the flexors of the digits did not predominate over the extensors in their action when the 6th lumbar nerve root was stimulated.

If the muscles producing flexion of a certain joint predominate in their action in one root, those producing extension predominate in another. This does not, of course, apply only to when both the opposing groups of muscles are represented in two nerve roots, but also when they are represented in different nerve roots. We have seen, for instance, that in the case of the ankle joint the muscles producing dorsiflexion are represented in the 5th and 6th lumbar nerve roots, while those producing extension at the same joint are represented in the 6th and 7th lumbar roots. This being the case, dorsiflexion results on excitation of the 5th lumbar root, and extension results on excitation of the 7th.

When two opposed movements are represented in three consecutive nerve roots, the middle root of the series is that in which both movements are represented, while the root above contains the one movement and that below contains the other. Sometimes the two move-

* *Cf.* Patterson, Forgue, &c., *loc. cit.*

ments represented in the middle root of the series cancel each other, as it were, so that neither predominates, as in the case of flexion and extension at the ankle as represented in the 6th lumbar root. But when the muscles producing the one movement predominate over those producing the opposite one, in my experience those muscles always predominate in that root, *e.g.*, the flexors of the digits in the 6th lumbar root.

As regards the order in which flexion and extension at the various joints are represented in their relationship to the spinal level, we find the following to be the order in the fore limb of the dog.

Flexion at the shoulder.*
Flexion at the elbow.
Extension at the shoulder.
Extension at the wrist.
Flexion at the wrist.
Extension at the elbow.
Extension at the digital joints.
Flexion at the digital joints.

Thus, while flexion is represented at a higher level than extension for the upper segments of the limb, the reverse obtains for the lower segments of the limb.

The following is the order of representation of these movements as regards the spinal level in the posterior extremity of the dog and monkey:—

Flexion at the hip.
Extension at the knee.
Flexion at the ankle.
Extension at the digital joints.
Flexion at the knee.
Extension at the hip.
Extension at the ankle.
Flexion at the digital joints.

Here the arrangement as regards the segments of the limb is an alternate one, flexion of the highest segment coming first, then extension of the next segment; while for the lower segments flexion again comes first, and is followed by extension for the next or terminal segment of the limbs.

So that in comparing the order of representation of movements of the posterior with that of the anterior extremity it is found that the highest segments coincide by having flexion as the highest representation, but that none of the other segments thus coincide, until the

* For purposes of comparison the forward movement of the limb at the shoulder joint is called flexion, while the backward movement is called extension.

terminal segments are reached, when extension is represented at a higher level than flexion in both instances. This is, however, not strictly accurate, for dorsiflexion at the ankle is strictly analogous to extension at the wrist; which leaves two joints alone at which there is any discord, viz., the elbow and knee.

It is possible by stimulation of a single bundle of fibres in a nerve root to produce contraction of a single muscle and it alone; but this effect is easier to obtain in the case of the cervico-dorsal roots which enter into the formation of the brachial plexus than it is in the case of the roots which combine to form the lumbo-sacral plexus, owing to the difficulty of isolating the separate bundles of nerve fibres of which the roots are composed for a great enough distance of their course after their exit from the neural canal, and before they unite to form the plexus in the case of the lumbo-sacral roots. The same muscle is always represented in more than one nerve root, usually two, and to an unequal extent in these. The only muscle which I have met with which is not represented in more than one nerve root is the tensor fasciæ femoris; and this agrees with Sherrington's observations with regard to this muscle. In coming to the conclusion that the rule is that a single muscle is represented in neither more nor less than two nerve roots, I wish it to be clearly understood that this conclusion is based upon the results obtained in any single individual of a class under observation. That, owing to variations, the same muscle may be found represented in three or even four nerve roots, I do not pretend to deny; but what I contend is that in the majority of instances a single muscle is represented in two nerve roots, and that when a variation is met with with regard to this muscle, it is, as a rule, that one of the nerve roots in which it is represented is different, rather than that it is represented in more nerve roots.

When the same muscle is represented in two nerve roots the muscle fibres innervated by one root are not innervated by the other; so that only part of the muscle contracts when a single root is excited. This part of the muscle may be either one end of it, one lateral half of it, or a superficial or deep part of it, as the case may be.

Ablation Experiments.

Division of any given nerve root produces paresis of the group of muscles supplied by it, which paresis is temporary, nearly all of it being recovered from. The amount of paresis or paralysis produced is proportional to the number of nerve roots divided; and this again varies according to whether the roots divided are consecutive or alternate ones, the effect being much greater in the former than in the latter case. That such should be the case is only what was to be expected, for section of any two consecutive roots cannot fail to cause

paralysis of certain muscles, if my observations with regard to the representation of muscles in the nerve roots are correct; whereas section of alternate roots, while causing paresis of more muscles, cannot produce paralysis of any one, the tensor fasciæ femoris being excepted. Such division of one or more nerve roots does not result in incoordination of the remaining muscular combinations represented in other nerve roots; the remaining movements are merely more feeble.

Exclusion of a certain Root or Roots during an Epileptic Spasm in the Limb.

Division of one or more nerve roots produces alteration of the position of a limb during an epileptic spasm, which altered position depends on the muscular combinations that have been thus thrown out of action. And the effect is identical when the root or roots are divided at the time that the convulsions are evoked, and when they have been divided some weeks previously, *i.e.*, the position assumed by the limb on one side of the body in which the root or roots are divided previously is identical to that assumed by its fellow of the opposite side, the root or roots of which are divided at the time that the convulsions are induced. No incoordination is produced in the action of the remaining muscular combinations; and there is no evidence of overflow of the impulses which ought to travel down the divided root into other channels through the spinal centres so as to reach the muscles by new paths.

LIST OF PREVIOUS EXPERIMENTAL RESEARCHES ON THE LUMBO-SACRAL PLEXUS.

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DESCRIPTION OF FIGURES.

Fig. 1.—From a photograph of the most common type of lumbo-sacral plexus met with in *Macacus rhesus*. Shows the lumbo-sacral cord, the nerve roots which

spring from one side of it, and the nerve trunks derived from these nerve roots. The 5th lumbar root sends a branch to the sciatic nerve in this type of plexus, and both the obturator and anterior crural nerves are derived from the 4th and 5th lumbar roots. The branch which appears to spring from the 3rd lumbar root and to pass to the 4th lumbar root, in the figure, is in reality a branch which has its origin from the 4th lumbar root, and which, together with the 3rd lumbar root, forms the external cutaneous nerve.

Sp.C. = Spinal cord.
I.L. = 1st lumbar nerve root.
I.S. = 1st sacral nerve root.
E.C. = External cutaneous nerve.
A.C. = Anterior crural nerve.
Obt. = Obturator nerve.
Sc. = Sciatic nerve.
Sc.′ = Division of sciatic nerve which supplies the hamstring muscles.

FIG. 2.—From a photograph of the type of lumbo-sacral plexus which is the most common variation met with in *Macacus rhesus.* The lumbo-sacral cord is represented with the nerve roots which arise from one side of it and the nerve trunks which have their origin from these nerve roots. The 5th lumbar root does not send a branch to the sciatic nerve, nor does the 2nd sacral nerve root do so. The obturator nerve receives a slender branch from the 6th lumbar root in addition to those derived from the 4th and 5th lumbar roots. The anterior crural nerve is formed from the 4th and 5th lumbar roots, while the external cutaneous is formed from the 3rd and 4th lumbar roots; and the 4th lumbar root receives a branch from the 3rd lumbar root, before it gives off any of its own branches.

Sp.C. = Spinal cord.
I.L. = 1st lumbar nerve root.
I.S. = 1st sacral nerve root.
E.C. = External cutaneous nerve.
A.C. = Anterior crural nerve.
Obt. = Obturator nerve.
Sc. = Sciatic nerve.

FIG. 3.—From a photograph of a lumbo-sacral plexus of *Macacus rhesus* of the same type as the last, intended to show how in some instances the 2nd sacral nerve root might be erroneously supposed to send a branch to the sciatic nerve, a fallacy which can only be avoided by more minute dissection.

Sp.C. = Spinal cord.
II.L. = 2nd lumbar nerve root.
I.S. = 1st sacral nerve root.
E.C. = External cutaneous nerve.
A.C. = Anterior crural nerve.
Obt. = Obturator nerve.
Sc. = Sciatic nerve.
Sc.′ = Division of sciatic nerve which supplies the hamstring muscles.

FIG. 4.—From another photograph of the most common type of lumbo-sacral plexus met with in *Macacus rhesus,* chiefly meant to show how slender the branch from the 5th to the 6th lumbar nerve root is in some cases.

Sp.C. = Spinal cord.
III.L., IV.L., V.L., VI.L., VII.L. = 3rd to 7th lumbar nerve roots.
I.S., II.S. = 1st and 2nd sacral nerve roots.
E.C. = External cutaneous nerve.
A.C. = Anterior crural nerve.
Obt. = Obturator nerve.
Sc. = Sciatic nerve.
× = Branch from 5th to 6th lumbar nerve root.

"A New Hypothesis concerning Vision." By JOHN BERRY HAYCRAFT, M.D., D.Sc. Communicated by E. A. SCHÄFER, F.R.S. Received February 16,—Read March 2, 1893.

(Abstract.)

It is suggested that many of the well-known facts of vision can be more easily understood when studied from the evolutionary standpoint. The eye is no exception to the general rule, accepted by evolutionists, that all parts of the body are gradually evolved under the environmental conditions of the species. Many species are devoid of a colour sense, but are able, nevertheless, to distinguish light from darkness, and where a colour sense is present it has been developed in relationship with environmental pigments : these points have been brought out with especial clearness by Darwin and Lubbock. We may infer, therefore, that the visual apparatus of a colour-seeing species—man, for instance—was at one time only able to distinguish light from darkness, and that the colours red, yellow, green, &c., were once seen as grey. This enables us to understand why it is that the outer, less used, parts of the retina are at the present day colour blind ; this fact fits in at once with our evolutionary hypothesis. From the same point of view we may explain why a minimal stimulus from a red, green, or other coloured object gives rise merely to the sensation grey—"bei Nacht sind alle Kätzen grau"—even when it falls upon the centre of the retina. In this case the minimal stimulus is unable to excite more than the simple sensation of light, and the quality of this light is not seen. A parallel may in fact be drawn between sight and hearing and smell, for we may hear a sound too feebly to assign to it its pitch, and we may have to sniff a faint odour in order to make out exactly what it is. But a red, yellow, or green object, very brightly illuminated, also appears white, and this has been explained in various ingenious ways. It is suggested, however, that this is merely a special case of

the law of *maximal stimulation*, which states that when a stimulus is increased beyond a certain amount it is not followed by any increased sensory effect. If you illuminate, say, a piece of red paper with an intensely brilliant light, it appears white, for the red pigment is unable to absorb all the blue and green spectral rays, which it would be able to do by a medium illumination, and enough of these green and blue rays are reflected to produce a maximum effect, and the red and yellow rays, though no doubt falling in greater quantity on the retina, produce likewise their full sensory effect and no more.

As the eye has been evolved by the action of the common pigments of nature, their examination throws light upon some of the facts of vision, and the sensory results of stimuli composed of certain mixtures of spectral rays may be explained from the evolutionary standpoint. If spectral rays near to each other, such as red and green, be mixed, their colour is that of the spectral ray which lies between them, in this case yellow. Now, when common natural pigments are observed spectroscopically, they are seen to transmit broad bands of spectral rays, generally extending to parts of the spectrum other than that part which corresponds in colour to that of the pigment. Thus a yellow natural pigment transmits a full flood of red, yellow, and green spectral rays. If we put it another way, the sensation yellow has in the course of evolution been produced by pigments which stimulate the eye by yellow spectral ray *plus* red and green spectral rays. These red and green spectral rays given out by natural pigments do not give rise to their respective sensations when mixed, for there is no such thing as a red-green sensation; but they intensify the yellow sensation which would be produced to a less extent by the intermediate yellow ray when acting alone. It is a *fact*, beyond which we cannot go, that the combination red *plus* green spectral ray stimulating the eye whenever we regard a yellow pigment produces the sensation we call yellow; an artificial mixture of such spectral rays of course gives rise to the same sensation. Similar explanations hold for the mixtures of green and violet, &c.

It is a fact that a sensation of white or grey is produced (*a*) when the eye is stimulated by all the spectral rays, (*b*) when it is stimulated, as shown by Helmholtz, by certain pairs, *e.g.*, red and blue-green. It is suggested that the colour top of Maxwell has, as a physiological experiment, been misinterpreted. When you mix on the disc a blue and yellow and get grey, the blue paper transmits to the eye one-half of the spectrum, viz., violet, blue, and some green, and the yellow paper transmits the other half, viz., some green, yellow, and red. You are therefore looking at what is physically the same stimulus as that given by a piece of white paper seen in half light. That the sensation grey occurs is not to be wondered at, for

the same stimuli give rise to the same sensation. That certain pairs of *spectral rays*, red and blue-green, for instance, produce grey or white is quite another fact, and may possibly be explained in the following way. All the pairs of spectral rays which together make grey or white are far apart from each other in the spectrum, and are not present in rays given out by any saturated pigment. Thus red and green rays stimulate the retina when any yellow object is observed, a pigment which gives out, in addition, the blue-green rays, is of a pale whitish-yellow. Thus, while pigments which give out red and green rays appear more yellow, those which give out red and blue-green rays appear less so and approach the primitive achromatic sensation.

Without knowledge of the changes which actually take place when light falls upon the retina, and before therefore the subject is really opened up, scientific observers have brought forward complete theories of vision. Both in the theory of Young and in that of Hering the visual organ is "conceived" by them, and in the absence of facts these theories can only be looked upon as tentative. In this paper an attempt has been made to arrange new facts by the side of old ones, in order that they may be understood the better. Beyond the point at which it is possible to explain a subject in terms of what we already know in physics and physiology, no progress has been attempted. Such attempts have in other departments of physiology proved too often unsuccessful to encourage effort in a subject the threshold of which every physiologist will agree that we are only about to enter.

"The Har Dalam Cavern, Malta, and its Fossiliferous Contents." By JOHN H. COOKE, F.G.S. With a Report on the Organic Remains, by ARTHUR SMITH WOODWARD, F.L.S., F.G.S., F.Z.S. Communicated by HENRY WOODWARD, LL.D., F.R.S., V.P.G.S. Received February 2,—Read February 23, 1893.

(Abstract.)

The Har Dalam cave is situated in the eastern part of the island of Malta, near Marsa Scirocco Bay. The headlands around the bay are composed of Lower Coralline Limestone, capped by *Globigerina* Limestone. Numerous valleys intersect the land at right angles to the coast line, forming small creeks and bays at their embouchure.

The Har Dalam gorge, in which the cavern is situated, is a valley of erosion which carries off the drainage of the land above, and was no doubt excavated at a time when the rainfall of Malta was much greater than at present. This is indicated by the heaps of rounded

boulders and water-worn *débris* in the gorge, and by the groovings and flutings on the rocks along its steep sides.

The cavern is situated 500 yds. from the shore on the north side of the gorge, and consists of a main gallery 400 ft. in length, when it ramifies in various directions, forming smaller tunnels and chambers, which follow the jointings and the bedding planes of the rock. One branch fissure is 250 ft. in length, 15 ft. high, and just wide enough for a man to pass along it, widening out at intervals into dome-shaped rock chambers. Two of the other galleries are of considerable length, but most do not exceed 20 ft. in length. One was traced for 35 ft., and ended in a fissure; another, having a N.E. course for about 100 ft., ended in a rounded extremity. They were filled to within 1 ft. 6 in. or 2 ft. of the roof with a reddish plastic clay, kept moist by percolation from the roof. The sides of all the galleries are in places encrusted with a stalactitic lining, generally obscured by a coating of clay.

The mouth of the main gallery is 26 ft. wide and 10 ft. high, and has been used during late years as a cattle shelter, the entrance being walled up and provided with a doorway. It widens inwards into a spacious chamber 60 ft. wide and 17 ft. high, having a branch to the right hand extending for 10 ft., but filled to the roof with boulders. The total length of the cave, including the terminal fissure, is 700 ft. The roof and sides are irregularly and smoothly arched, but the height and width vary considerably, as shown by section and plan.

The stalactites which largely covered the roof have been mostly broken off by later torrential action, but the larger ones, 3 to 6 ft. in circumference, still remain *in situ*. Raised bosses of stalagmite on the floor correspond with the stalactites above. These stalagmitic bosses were observed at three different levels, each being covered by fresh alluvial deposits, indicating the intermittent character of the floods that invaded the cavern and the long periods that elapsed between them.

The present floor of the main gallery is fairly even. After entering there is a rapid descent, followed by a gentle rise to the extreme end of the gallery. The deposits met with vary considerably in different parts of the cave. At the furthest extremity they are mainly composed of red loam; in the middle, of large boulders and broken stalactites and old pottery enclosed in clay; towards the entrance, of a grey indurated marl, with abundant remains of land shells, roots of plants, bones of Deer, and boulders. The cave is everywhere strewn with enormous quantities of water-worn boulders similar to those met with so abundantly in the valleys and gorges of the islands.

Excavations show that the sides of the cave slope inwards, and form a trough-like rift of very irregular outline and much broken and fissured.

It is the author's opinion that the cave owes its present dimensions to the same torrential waters which widened the gorge and moulded and channeled the rocky sides of both.

It must have been repeatedly submerged, and its sides became coated by the muddy waters with the clay now adhering to them.

These torrential inundations no doubt caused the death and entombment of the Hippopotami, Deer, and other animals, whose remains, commingled with boulders and broken-off stalactites, attest the turbulence of the waters.

Eight excavations were made in various parts of the cavern, of which ground plans and sections are given by the author.

Trench I was opened on the right-hand side of the main gallery, 350 ft. from the entrance.

It was 24 ft. in length, 8 to 9 ft. in width, and 8 ft. 6 in. in depth. It gave the following deposits *in descending order*:—

A. Floor earth with stones, clay, and boulders (unstratified), 6 in.

B. Red clayey loam (3 ft. in thickness), containing bones of Hippopotami (*H. Pentlandi*), teeth and bones of *Cervus*, the bones very irregularly distributed; also some fragments of ancient pottery.

C. Layer of black earth (4 in. thick), like coarse oatmeal, quite unfossiliferous, but very persistent throughout.

D. Dark red plastic clay (1 ft. 6 in. thick), with many remains of *Hippopotamus Pentlandi* in good preservation and less disassociated. Although well preserved, however, they were so soft from the dripping of water that they would not bear handling.

E. Layer of reddish clay (1 ft. thick) full of bones, jaws, and teeth of a small *Hippopotamus*, cemented together by calcareous infiltrations, and forming an ossiferous breccia (very compact). A single molar of *Elephas* (*E. mnaidriensis*) was found here.

F. Stratum of stiff yellow plastic clay (2 ft. thick), unfossiliferous, but enclosing a few angular fragments of limestone fallen from the roof.

Trench II, also on the right-hand side of the cave, but nearer to the entrance, showed a series of ten regularly stratified layers, mostly alternating layers of clay and grey calcareous marly earth.

The upper layers contained no organisms, but the red-marly earth contained numerous remains of antlers of Deer and abundance of land shells (but mostly broken); beneath this series, and separated by a seam of marly earth, is a bed of red clay (3 ft. thick), containing antlers, teeth, and bones of Deer, and remains of *Hippopotamus*, very well preserved.

Trench III, midway, in the centre of the floor of the cave: this trench reached the rock at 6 ft. 6 in. from the surface. Immediately in contact with it is a layer of dark red clay, with broken bones of

Hippopotamus, but no Deer. The fissures in the rock itself also contained remains of *Hippopotamus* firmly wedged and cemented into them.

Trench IV, also in the centre of the cave, exhibited a series of deposits similar to those in Nos. I and III.

Layers A, B, and C, in descending order, contained no organic remains, but in layer D (indurated brick-red earth), which appears to be the basal layer of C, composed of its heavier materials, sorted by water, are remains of *Hippopotamus* and *Cervus* intermixed in pell-mell confusion. Amongst these remains Mr. Arthur Smith Woodward has determined the third metacarpal of Man. It was found at depth of 3 ft. 6 in. from the surface, and underlying a layer containing pottery. It is probably of great antiquity, being extracted from one of the earliest layers in the cavern. The layer beneath contains bones of Cervidæ and Hippopotami, but much broken.

Trench V was in a rocky fissure on the left-hand and furthest from the entrance of any of the trenches. It was excavated to a depth of 8 ft., but few organic remains were met with in it; there were many limestone boulders, some fragments of pottery, and several pieces of bones of *Hippopotamus.*

Trench VI, made on the left hand, 50 ft. from the entrance, is interesting because, in layer E, consisting of light gray clay and loam, with a few antlers, teeth, and bones of *Cervus*, was found the first Carnivore yet met with in Malta; represented by a jaw and several canines of *Ursus.*

In the layer beneath were numerous bones and teeth of the small *Hippopotamus.*

Although Admiral Spratt and Dr. Leith-Adams refer to the probable presence of Carnivora (from the gnawed condition of the bones in the Zebbug gorge), these are the first remains ever met with.

Trench VII was cut in a small branch cavern, running about 15 ft. into the rock at right angles to the main gallery, about 20 ft. from the main entrance.

Here, beneath a mass of boulders, was a layer of dry black earth, intermixed with grass, and fragments of pottery.

The layer beneath contained large blocks of limestone embedded in light friable marl, with great quantities of shells of Helices, jaws and bones of Sheep, &c.

Trench VIII, made within a walled enclosure 30 ft. from the entrance, was not rich in organic remains; but antlers and limb-bones of Deer and remains of *Hippopotamus* marked the corresponding layers met with in the other trenches.

The author concludes that the Har Dalam Valley is identical in its characteristics with the other valleys and gorges of Malta and Gozo, and that they all owe their origin to analogous causes: chiefly, he

thinks, to the action of marine erosion during a period of depression and of re-elevation; but *not* altogether to this cause.

The limited extent of the present land area, and of the annual rainfall it enjoys, would certainly not afford fresh water for a stream sufficient to cut out these gorges (often hundreds of feet in vertical depth). The author therefore considers they must date back to a period when these islands formed part of a much larger (indeed a continental) area, and that the rainfall must have been *much greater* than it is at the present time.

That during periods of torrential rains entire herds of Herbivora must have been drowned, and their carcasses swept into the gorge, and thence into the Har Dalam and other caves, where their remains were embedded.

In evidence of the great antiquity of these deposits, he points to the fact that the mouth of the cave is now 40 ft. above the bed of the small stream which, in rainy weather, flows through the gorge, and that no torrential waters now sweep through it; and, further, that the cutting down of 40 ft. of rock must have been an extremely slow and gradual process, and have taken place since the last cave layer was deposited.

Of the pottery, two well marked kinds were observed: the one, a rude, coarse, unornamented fabric; the other, finer in texture, and characterised by markings similar to those found on ware which occurs in tombs at Malta, and known to be of Phœnician and Punic origin.

The earlier kind is within 2 or 3 ft. of the surface; the later, only in the superficial layers.

Report on the Organic Remains. By ARTHUR SMITH WOODWARD, F.L.S., of the British Museum.

I. CAVE EARTH.

Ursus arctos (?).

The only remains of Carnivora discovered in the cave earth (Excavation No. VI, layer ii) comprise the imperfect left mandibular ramus and left lower canine of a small Bear, and another canine tooth equalling in size that of a Wolf. The mandibular ramus of the Bear belongs to a fully adult, perhaps aged animal, the permanent dentition being well worn; and the detached canine indicates a mandible of the same size. There is a small socket behind the canine for pm. 1; and pm. 4 is well developed, but without any inner tubercle beyond a rudiment anteriorly. The length of the diastema is 0·038, and the teeth preserved measure in length respectively as follows*:—

* All measurements are given in decimal fractions of the metre.

pm. 4.	m. 1.	m. 2.
0·014	0·023	0·025

It is impossible to determine a species of *Ursus* by the imperfect detached mandible; and the only certain statement that can be made

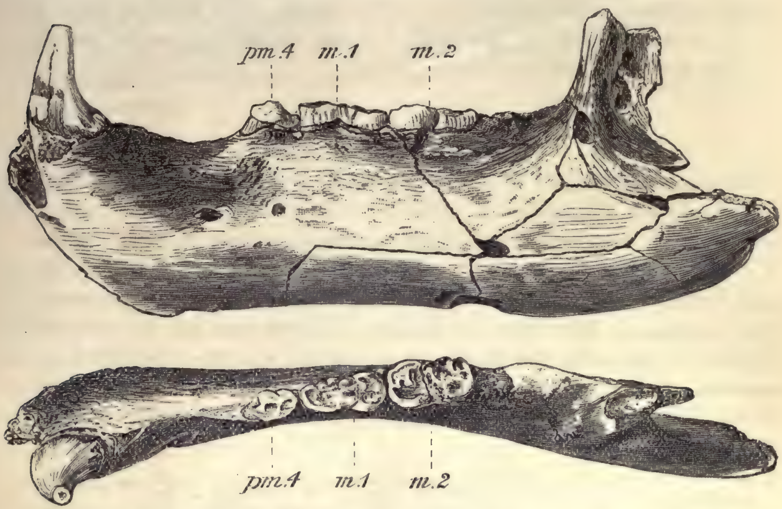


FIG. 1.—Left Mandibular Ramus of *Ursus arctos* (?); one-half natural size.

in reference to the present fossil is that it does not represent a dwarfed form of the extinct Cave Bear. This is proved by the comparatively simple character of pm. 4. The specimen agrees precisely in size, form, and proportions with the mandible of a common Brown Bear (*Ursus arctos*) in the British Museum numbered 218, *d*; and hence the Maltese form may be provisionally assigned to this species until the discovery of further remains. The jaw is much smaller than those from the Gibraltar caverns assigned by Busk to a variety between *Ursus arctos* and *U. fossilis*.

Canis *sp.*

The small canine tooth already referred to belongs to the left side of the mandible, and probably represents a species of *Canis* equalling the Wolf in size. Whether or not it is referable to a domestic animal is as yet indeterminable.

Elephas mnaidriensis.

Remains of Elephants are very rare in the Har Dalam cavern only an imperfect humerus and a molar tooth having been dis-

covered. Of the humerus, which belongs to the left side, the distal two-thirds of the shaft is alone preserved without either extremity; and it can only be said that the bone indicates an animal of the size of *Elephas mnaidriensis*. To this species, however, may be ascribed with certainty the molar tooth, which resembles in all respects the upper molar 2, as described by Leith Adams. The tooth is worn and fixed in a fragment of the jaw, but its anterior portion has been broken away beyond the fourth plate.

Hippopotamus Pentlandi.

Bones and portions of dentition of *Hippopotamus* form nearly the whole of the collection of remains from the cave earth; and it is noteworthy that nearly all these remains pertain to adult animals. They are much broken, and very similar in character to the bones and teeth of *Hippopotamus Pentlandi* discovered by Dr. Falconer in the Grotta di Maccagnone, Sicily. There can be no hesitation, indeed, in assigning the Maltese form to this species; and the following few maximum measurements, made so far as the fragmentary bones will allow, indicate its small dimensions as compared with *H. amphibius*:—

Length and breadth of glenoid facette of scapula..	0·085 × 0·073
Width of trochlear articulation, distal end of humerus ..	0·09
Length of radius	0·235
Width of proximal articulation of radius	0·083
,, distal ,, ,, ...	0·110
Length and breadth of acetabulum	0·079 × 0·085
Width of proximal end of femur	0·155
,, distal articulation ,, ...	0·14
,, proximal articulation of tibia.................	0·146
Astragalus	0·09 × 0·075 × 0·05
Length of calcaneum	0·17

Cervine Remains.

From a superficial layer of the cave earth in excavation No. IV, numerous much mineralised remains of Deer of various sizes were obtained. All the bones and antlers are rolled and too fragmentary for specific determination; but the majority may well have belonged to the Barbary Deer, of which there is good evidence in the overlying deposit. Several fragments prove that the antlers were not shed specimens, and one base exhibits the insertion of two brow tynes such as characterise the adult *C. elaphus*.

Other fragmentary Cervine remains, including one pedicle and burr of antler, were discovered in the cave earth in Excavation No. VI

(layer iii), in the same condition as the associated bones of *Hippopotamus*, and bones of Deer also occurred in association with the mandible of Bear noticed above (Excavation No. VI, layer ii).

Human Remains.

A single human metacarpal III is also contained in the collection from the superficial layer of cave earth in Excavation No. IV. It is mineralised apparently to a somewhat less extent than the associated bones of animals.

II. Superficial Deposits.

Man and Domestic Animals.

Associated with the rude pottery discovered in the surface deposit in Excavations Nos. II and IV are various traces of a small Pig and a Goat or Sheep, besides a single tooth of *Bos*. A Deer larger than the variety of Barbary Deer mentioned below is also represented by numerous limb bones and some teeth. In an equally superficial layer in Excavation No. VI there is a single metatarsal of a Dog as large as a Wolf, and an imperfect cannon bone of a very small Horse or Donkey.

Bones of Bats have evidently been accidentally introduced, and there are a few fragmentary rolled remains of *Hippopotamus* derived from the Cave Earth. A few fragments of a small land Chelonian also occur.

Cervus elaphus, *var.* barbarus.

The remarkable accumulation of bones in the dry stalagmitic superficial layer in Excavation No. VI consists almost entirely of remains of a small Deer in all stages of growth, perhaps even from the unborn fœtus onwards. None of the bones exhibit evidence of gnawing or artificial fracture, and although some of the shed antlers appear to have been gnawed, it is probable that these have been bitten by the Deer themselves in accordance with their usual custom.

Nearly all the antlers are distinctly shed specimens, and the largest complete example measures 0·63 in total length of the beam. This antler bears two short brow tynes, and shows only one bifurcation of the beam above; but the terminal fragment of another specimen, evidently of an older individual, exhibits a second bifurcation at the apex of the beam. There is no evidence in any fragment of an expanded crown or palm. Some of the smaller antlers have only one brow tyne, and in one malformed case this is abnormally bifurcated. A simple pricket, wanting the extreme tip, measures 0·2 in length. The surface in all specimens is more or less ridged and furrowed longitudinally, and there is a slight burr at the base.

FIG. 2.—Antler of *Cervus elaphus*, var. *barbarus*; one-fifth natural size.

Jaws are numerous, and there are a few portions of skull, apparently broken during excavation. The following measurements of the teeth indicate their maximum size, and will serve for comparison :—

Length of upper	m. 1, 0·013;	m. 2, 0·017;	m. 3, 0·017.
" lower	pm. 2, 0·010;	pm. 3, 0·012;	pm. 4, 0·012;
	m. 1, 0·014;	m. 2, 0·017;	m. 3, 0·023.

Of the very variable limb bones the following maximum and minimum measurements of length in adult specimens are worthy of record :—

	Maximum.	Minimum.
Length of humerus	0·19	0·17
" radius	0·215	0·175
" metacarpus	0·19	0·16
" femur	0·215	0·21
" tibia	0·26	0·22
" metatarsus.........	0·203	0·18
" calcaneum	0·095	0·063
" astragalus.........	0·04	0·035

The small dimensions of all these Cervine remains suggest a comparison at first with the common Fallow Deer (*Cervus dama*); and it is quite possible that some specimens—notably those from Excavation No. IV—may represent this southern European form, which has already been recognised by Busk in the caverns of Gibraltar. The limb bones, however, appear to the present writer to be slightly more robust than those of the Fallow Deer of corresponding size; and the antlers conclusively prove that most of the remains, at any rate, do not belong to this species. The antlers may be assigned with certainty to the small variety of *Cervus elaphus* which now lives in Northern Africa, and is known as the Barbary Deer (*Cervus barbarus* of Gray); the Maltese fossils, however, indicate an animal of smaller dimensions than its existing representative and its contemporaneous ally discovered in the caverns of Gibraltar.

"The Effects of Mechanical Stress on the Electrical Resistance of Metals." By JAMES H. GRAY, M.A., B.Sc., and JAMES B. HENDERSON, B.Sc., "1851 Exhibition" Science Scholars, Glasgow University. Communicated by LORD KELVIN, P.R.S. Received February 10,—Read March 2, 1893.

This investigation was begun under the instructions of Lord Kelvin about a year ago, and has been continued since the beginning of last year in conjunction with another on thermal conductivity, for which a grant of £50 was made from the Government Research Fund.

The chief object of the investigation was to obtain quantitative results of the variations of specific resistances of metals due to stretching, twisting, drawing through holes in a steel plate, hammering, heating, and combinations of these, while in some of these cases the alteration of density was also measured.

The most exhaustive results that have been hitherto given in this direction are those of Lord Kelvin, published in vol. 2 of his 'Re-

print of Mathematical and Physical Papers'; of Dr. A. Matthiessen, F.R.S., published in the 'British Association Reports' for 1862, 1863, 1864, and 1865; and those of Mr. Herbert Tomlinson, F.R.S., given in several papers communicated to the Royal Society in 1877 and subsequent years. The paper of the last, dealing most particularly with the present investigation, is contained in the 'Phil. Trans.,' 1883, pp. 1—72, "On the Influence of Stress and Strain on the Action of Physical Forces."

As this work was done at two different periods of time, it has been found convenient to divide the paper into two parts. Part I, which contains the results of change of density due to the different kinds of treatment, was done previous to last July. Part II contains the results of alteration of resistance due to stretching, the preliminary work and trial methods, which occupied a very considerable time, having been done in conjunction with the work of Part I.

Part I.

By James H. Gray.

Density.

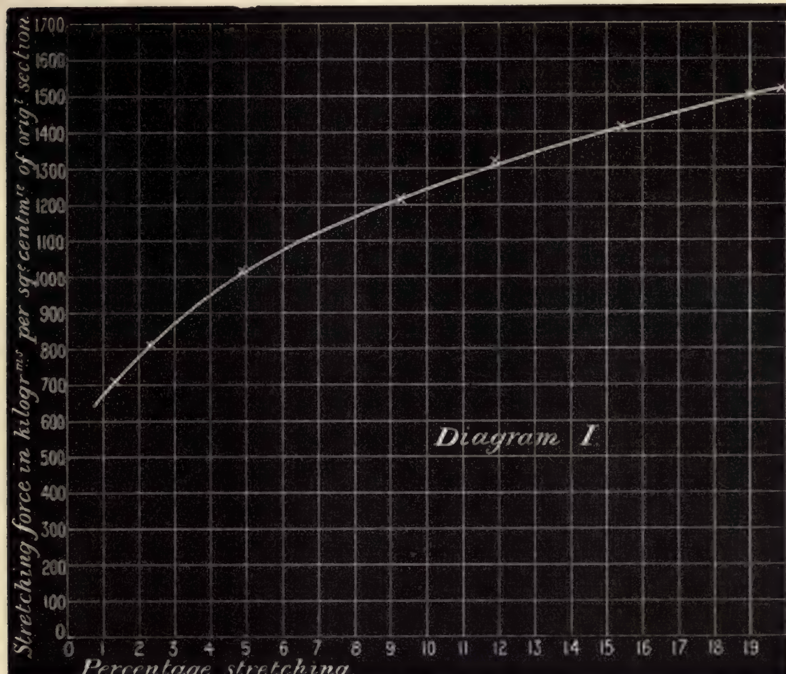
As, in every case, the alterations were expected to be small, great care had to be taken to have all the apparatus as sensitive as possible. A very delicate Oertling balance was used, capable of weighing accurately to within $\frac{1}{10}$ milligramme, and all the usual precautions observed.

Effect of Stretching.

A well-annealed wire of practically pure copper, electrical conductivity 98 per cent., diameter 2 mm., was stretched by weights till it broke. The wire was securely fixed to a strong hook near the ceiling of the laboratory, and two ink marks made, one near the top end, the other near the bottom end. Behind these marks were fixed $\frac{1}{2}$-mm. scales, which enabled the stretching to be recorded. After the wire had been stretched by a weight, a length of about 8 inches was cut off. In this manner successive weights were applied, pieces of the wire being cut off each time, till it broke. Even when so great lengths as 8 inches were taken however, it was found very difficult to weigh very accurately in water, and this, along with possible differences, due to the manner in which the several tests were made, may account for the fact that the intermediate values of the densities varied in different series. The original and final densities of the wire could always be determined very accurately, as much greater lengths of the wire could be used. The numbers below give the values, which were found to be very constant in all the specimens tested.

Original density before stretching = 8·8612
Density after stretching almost to breaking = 8·8187

This shows a decrease in density of fully $\frac{1}{2}$ per cent., and is somewhat greater than what Mr. Tomlinson obtained for the copper which he tested, his result being $\frac{1}{3}$ per cent. In the present tests, the stretching obtained was usually about 25 or 26 per cent. before breaking. Diagram 1 is a stress-strain curve of the copper wire used, the abscissæ denoting percentage strain, the ordinates the stress in kilogrammes per square centimetre of the original section of the wire. This curve is given to show the nature of the wire.



Lead Wire.—A similar series of tests was made on a length of lead wire, diameter 0·8 mm., the stretching being carried on till the wire broke. This point was reached after a stretching of 3·6 per cent. The values for the original and final densities are as follows :—

Percentage stretching.	Density.
0 (original wire)	7·695
3·6*	7·637

This shows a diminution of density of ½ per cent.

Effect of Drawing through Holes in a Steel Plate.

Copper Wire.—A length of the same copper wire as was used before, diameter 2 mm., was drawn, without any special care, through twenty holes of uniformly diminishing diameters, the density being measured after drawing through every third hole. Table I gives the results obtained. It will be seen that, whereas in stretching the density was diminished, in drawing it is considerably increased. There would seem to be a maximum after drawing through twelve holes, but as the wire became very difficult to draw by this time, and broke after being put through six more holes, the subsequent decrease in density may have been due to flaws in the metal, caused by the rough treatment which it had received. The ultimate diameter was 1·3 mm.

Table I.

	Density.
Before drawing	8·85
After drawing through three holes	8·87
" " " more holes	8·92
" " " "	8·95
" " " "	9·005
" " " "	8·94
" " " "	8·92

Manganese-Copper.—A length of this alloy (10 per cent. manganese, 90 per cent. copper), diameter 1·2 mm., was drawn through several holes, till the diameter was reduced to 0·6 mm., the results for the original and final densities being as follows:—

Density of original wire = 8·53
" after being drawn = 8·615

Effect of Twisting.

A length of 130 cm. of 98 per cent. electrical conductivity wire, diameter 2 mm., was fixed at one end to a support, and a weight of

* Wire broke.

56 lbs. attached to the other end. While this weight was on, 300 complete turns were made in the wire, the density being measured after 200 turns, and again after 300 turns. It was found that the wire had stretched by about 23 per cent. after being thus treated. The density, as will be seen, increased very slightly—about $\frac{1}{2}$ per cent.

Density before twisting = 8·850
„ after 200 turns = 8·887
„ „ 300 „ = 8·896

A twist of 300 turns in a length of 130 cm. represents 2·3 turns per centimetre.

This wire was also tested for alteration of torsional rigidity and Young's modulus by Mr. J. E. Monroe, with the following results:—

Length of wire = 425 cm.

Number of twists.		Torsional rigidity, grammes per square centimetre.	Young's modulus, dynes per square centimetre.
Twists put in.	Permanent twists.		
0	0	$4·07 \times 10^{11}$	$1·116 \times 10^{12}$
5	0	$4·07 \times 10^{11}$	$1·152 \times 10^{12}$
10	—	$3·994 \times 10^{11}$	$1·17 \times 10^{12}$
15	—	$3·947 \times 10^{11}$	$1·152 \times 10^{12}$

Heating.

A preliminary trial was made on the effect of heating. The wire which had been used in the experiments just described was raised to a white heat by an electric current, to find out if by this means the density could be brought back to its original value which it had before being twisted. The heating, however, did not seem to alter the density appreciably, the difference not being more than $\frac{1}{10}$ or $\frac{1}{15}$ per cent.

Effect of Hammering.

A piece of copper wire was flattened by heavy blows with a hammer, and the density measured. The hammering was then continued, and the density again measured.

Density of original wire = 8·866
„ after first hammering = 8·868
„ „ second hammering = 8·875

Part II.

By James H. Gray and James B. Henderson.

Tests on Change of Electrical Resistance.

Several methods were tried with more or less success at the beginning of the work of Part I. Great difficulty was at first experienced with thermo-electric currents and the self-induction of some of the coils used. Ultimately, the zero method about to be described was perfected, and was used in the work of Part II, giving great satisfaction.

Before describing this method, we wish to refer particularly to the two definitions of specific resistance at present employed. The unit most generally understood in English treatises is the resistance in ohms of a cube of the metal of unit section and unit length. From this definition we have, for the resistance R, of a length l, of a homogeneous conductor of uniform section ω,

$$R = \sigma_v l/\omega,$$

where σ_v is the specific resistance so defined. σ_v may be called the "volume specific resistance," in contradistinction to the "weight specific resistance" σ_w, which is defined from the following. We have

$$R = \sigma_v \frac{l}{\omega} = \sigma_v \rho \frac{l^2}{l\omega\rho} = \sigma_w \frac{l}{w} l,$$

where $\sigma_v \rho = \sigma_w$, w being the weight of the length l of the wire. From this, σ_w is seen to be the resistance of a length of the wire numerically equal to ρ and section unity, or of a length unity and section equal to $1/\rho$. Since the section is uniform, l/w, or the length per unit of weight, is constant. Let it be represented by λ. Then we have

$$R = \sigma_w \lambda l.$$

The advantages of using this latter equation over the one involving the "volume specific resistance" are very many, either when it is required to know the whole resistance of a wire, having given σ_w, λ, and l, or when it is required to know σ_w, having given R, λ and l.

It is quite usual in commercial circles to speak of a wire of, say, number 14 gauge, weighing 127 grains to the foot, that is, about 27 grammes to the metre. The only measurement to be taken then is the length, if the specific resistance be given in weight units, and the measurement of the length can be made with the greatest accuracy. Even if the weight per unit of length be not given, it can also be determined most accurately without any difficulty.

If, however, the specific resistance be given in volume units, the

section of the wire must be measured, and this is a very difficult thing to do, even in the case of moderately thick wires, when accuracy is required, since an error in the measurement of the diameter is more than doubled in the value of the section. Everyone who has tried to measure accurately the diameters of wires is well aware of the great difficulty in doing so. Even although the one measurement were quite accurate, the diameter at the place measured would, in all probability, not be the average diameter of the wire. This can, of course, be corrected by taking measurements at a number of places, and taking the average, but the process is very tedious, and, when done, is not thoroughly trustworthy, for it is so easy to make errors in using the ordinary micrometer gauge. If the section be determined by the longer method from a measurement of the density, it is, of course, more accurately obtained, but, even then, the weighing in water of small lengths of wire is an uncertain thing; whereas, for the length per unit of weight, the weighing has only to be made in air, and therefore the error due to weighing in water is avoided. Clearly, then, the "weight specific resistance" gives much more accurate results for the total resistance of a wire than the "volume specific resistance," particularly in the case of practical work, where very little care is taken in measuring the diameter. Certainly, it would have been incomparably more difficult, and would have taken a much longer time to obtain the results given in this paper, had the "volume specific resistance" been used. As will be shown further on, it is not necessary, for mere comparison of two specific resistances, even to measure the length per unit of weight.

Dr. A. Matthiessen, F.R.S., in his paper "On the Specific Resistance of Metals in terms of the British Association Unit (1864) of Electric Resistance, &c." ('Phil. Mag.,' May, 1865), says:—

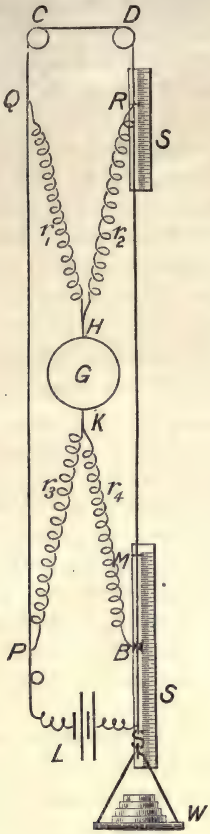
"We employed the length weight in preference to the length section, knowing that the weight of a wire may be much more accurately determined than its section, whether deduced directly from the measurements of the diameter, or indirectly from the specific gravity, the determination of the latter introducing an error. Of course, in endeavouring to reproduce resistances, it is wise to avoid the use of unnecessary values; and it is just as well, and certainly a much more accurate method, to determine a resistance in length weight than in length section."

The term "specific resistance" was introduced by Weber, but by it he meant the weight unit, and always used it unless when otherwise stated. Lord Kelvin points this out in his paper on "Measurement of Electromotive Forces in Absolute Measure" (vol. 1, 'Math. and Phys. Papers'), and throughout three papers relating to the present investigation, on "Electromagnetic Qualities of Metals," "Analytical

and Synthetical Attempts to ascertain the cause of the Differences of Electrical Conductivity discovered in Wires of nearly Pure Copper," and "On the Electric Conductivity of Commercial Copper of various kinds," printed in his collected papers, the "weight specific resistance" is always used. Several of the most eminent authorities have from time to time signified their disapproval of the volume unit, but it still continues to be most generally used. Why this is so does not seem clear, for surely a much more definite idea of a metal is obtained when its density is taken into account as in the weight unit; and if, for certain purposes, the volume unit may be convenient, this can easily be found from a knowledge of the density. It has been conclusively established, first by Lord Kelvin, and afterwards by several investigators, that the volume specific resistance always increases with the decrease of density, and therefore the weight resistance, since it includes the density, will not change so much. The weight unit is not, however, quite constant, as the results of this investigation show, but the changes, at least for copper, iron, and steel, are very small. As a matter of fact, for these metals the volume specific resistance does not change very much either, as the density is practically constant for any mechanical treatment.

The method used for the test of change of specific resistance was a slight modification of that known as Thomson's (Lord Kelvin) Double Bridge Method ("New Electrodynamic Balance for Resistance of Short Bars or Wires," 'Phil. Mag.,' 4th Series, vol. 24, 1862, p. 149). Diagram 2 is a sketch of the arrangements. A length of wire, PCDB, of about 10 metres, was fixed about its middle point round two strong bolts, C and D, which were fixed firmly near the ceiling in a pillar of the physical laboratory. At the points P, Q, R, the ends of 150 ohms resistances, r_1, r_2, r_3, were neatly soldered, the other ends of these resistances from Q and R being brought to one terminal, H, of a Thomson's mirror galvanometer G, of resistance 5380 ohms, the end of the resistance from P being connected to the other terminal K of the galvanometer. From K another 150 ohms resistance, r_4, was carried to a sliding contact, B. The four resistances of 150 ohms each were inserted so that practically all the current from the battery L would flow in the circuit PCDB, and thus any movement of B will not sensibly disturb the distribution in this circuit. As will be at once seen, if the resistance of the wire PQ is equal to that of BR, there will be no deflection in the galvanometer. The wire PQ was left unaltered, and served as a standard of comparison for BR, which was subjected to successive stretchings by means of weights, W. Half-millimetre scales, S, S, fixed immediately behind the points R and B, enabled the readings of length, BR, to be accurately taken.

DIAGRAM 2.



The order of an experiment was as follows:—The test wire BR was first of all made as straight as possible by means of a small weight. The sliding contact B was then moved about till there was no deflection in the galvanometer. Readings were taken at the points B, R, and also at a pointer, M, fixed on the wire near the top of the bottom scale S. A weight of 7 lbs. was then added to the small weight, and a balance again taken. The additional weight was then taken off, and a balance found. In this way, successive additions of 7 lbs. were made, and balance readings taken each time till the wire broke. The distance between the wires was about 10 cm., and the length between B and R from 400 to 450 cm. The galvanometer was made so sensitive that a movement of B of $\frac{1}{4}$ mm. could be distinctly detected, so that a change of $\frac{1}{4}$ in 4000, that is, 1/160 per cent. was measurable, and allowing for small errors in

reading the knife-edge of B, the result could be easily obtained correct to $\frac{1}{30}$ per cent. The method being a zero one, the galvanometer could be made almost unstable, and, as there are over 23,000 turns on the coil, the arrangement was exceedingly sensitive. The maximum current used was 0·5 ampère, and, in the case of iron and steel wire, not more than 0·25 ampère.

The four resistances r_1, r_2, r_3, and r_4, of 150 ohms each, were carefully wound anti-inductively side by side on a piece of slate, and covered with cotton tape, to ensure their being at the same temperature. The whole system of wires was so arranged as not to influence the galvanometer. The distance between the standard and test wire being so small, the temperatures of both were the same, and the sliding contact, at the point where it had to be touched by the hand, was protected by a piece of vulcanite from being heated. With these precautions, no inconvenience was experienced from thermo-electric currents.

Calculation of Results.

From the method of calculation it will be seen that it was only necessary to measure lengths in order to obtain results of the variations of the weight specific resistance, and to determine densities in addition, when the volume resistance was also required. The measurement of so great lengths as 400 cm. could be made very accurately, and, therefore, very little error was introduced. In no case was it necessary to measure the section in connexion with the resistance.

Let l = original length of the test wire which would give a balance with the standard, l' = the length which would give a balance after applying weight to it, L = the original length between R and M before applying weight, ω = the section before applying weight, L$'$, ω' the corresponding values after applying weight,

σ_v = volume specific resistance,
σ_w = weight specific resistance,
R = resistance between the points P and Q on the standard.

Then, since the sliding contact on the test wire is always adjusted till the resistance is equal to R_1, we have

$$R = \sigma_v \frac{l}{\omega} = \sigma'_v \frac{l'}{\omega'},$$

$$= \sigma_v \rho \frac{l}{\omega\rho} = \sigma'_v \rho' \frac{l'}{\omega' \rho'},$$

where ρ = density before applying weight,
ρ' = " after " "

But, since the weight of the length between the two fixed marks, R and M, on the wire, remains constant, we have—

$$\text{weight} = \mathrm{L}\omega\rho = \mathrm{L}'\omega'\rho' ;$$

$$\therefore \frac{\omega'\rho'}{\omega\rho} = \frac{\mathrm{L}}{\mathrm{L}'} ;$$

$$\therefore \frac{\sigma'_w}{\sigma_w} \equiv \frac{l'_v\rho'}{\sigma_v\rho} = \frac{l}{l'} \times \frac{\omega'\rho'}{\omega\rho} ;$$

$$\therefore \frac{\sigma'_w}{\sigma_w} = \frac{l}{l'} \times \frac{\mathrm{L}}{\mathrm{L}'} .$$

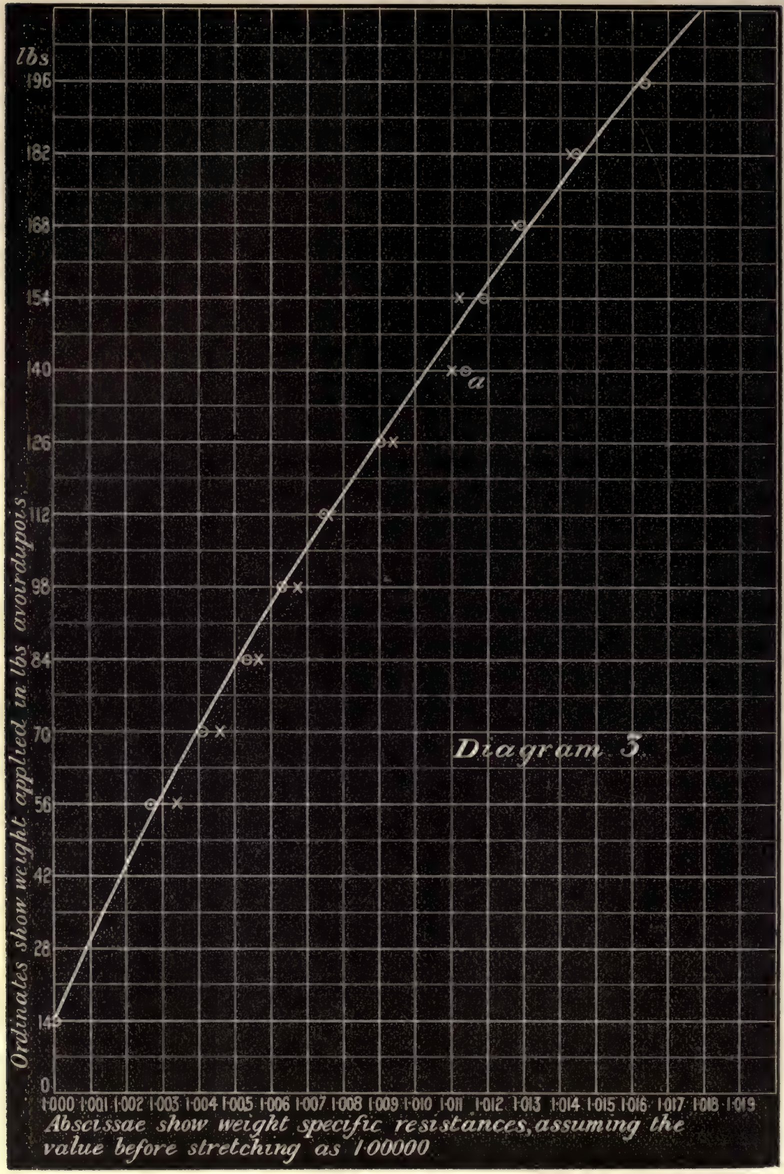
Thus, when a balance has been found, it is only necessary to measure, by means of the scales S_1 S, the lengths l' and L$'$, the other two, l and L, having been read before the weights were applied. The ratio of the weight resistances after and before stretching is thus obtained. If the ratio of the volume resistances is required, we have $\sigma'_w/\sigma_w = \sigma'_v\rho'/\sigma_v\rho$, so that, by cutting off suitable lengths of the wire and determining their densities, we get σ'_v/σ_v.

The preliminary trials of this method were made on copper wire with the help of Mr. Hamilton Wingate. The results obtained quite agreed with what Lord Kelvin first, and Mr. Tomlinson afterwards, found, that the mechanical treatment did not materially affect the specific resistance.

Tests of Steel Wire.

Pianoforte steel wire, of diameter 0·8 mm., was used, and straightened by a weight of 14 lbs. An additional 42 lbs. weight was

Column 1.	Column 2.	Column 3.	Column 4.
Weight applied in lbs. avoirdupois.	Ratio of weight specific resistance with weight on to weight specific resistance before applying any weight.	Ratio of weight specific resistance with weight on to weight specific resistance after the weight has been taken off.	Ratio of weight specific resistance after weight was taken off to that before any weight was appplied.
14	1·0000	1·0000	1·0000
56	1·0027	1·0034	0·9993
70	1·0041	1·0046	0·9994
84	1·0054	1·0056	0·9998
98	1·0064	1·0067	0·9997
112	1·0075	1·0076	0·9999
126	1·0091	1·0095	0·9996
140	1·0115	1·0110	1·0004
154	1·0119	1·0112	1·0006
168	1·0130	1·0129	1·0000
182	1·0145	1·0144	1·0001
196	1·0163	1·0165	0·9999



Curve of results given in Columns 2 and 3.

added, and a balance found. This was then taken off, and a balance again taken.

The calculations from these readings give the temporary altera-

tion of specific resistance due to the strain caused by the weight, and the permanent alteration due to the weight having been applied for about ten minutes, and then taken off. This series was continued by making successive additions of 14 lbs. till the wire broke, which it did when a little more than 200 lbs. had been added. The results obtained for one of four series are given in the table, p. 293. This series was chosen because more intermediate readings had been taken than in the other three. All four agreed at the common points within the limits of accuracy of the method.

The headings of the columns sufficiently explain what the numbers mean. Column 4 shows that application of weight up to 84 lbs. had the effect of slightly improving the permanent electrical quality of the steel. When more than 84 lbs. were applied the conductivity was found to diminish again. The results in column 2 are shown plotted on the curves in Diagram 3. Columns 2 and 3 are almost identical, as shown by column 4, which is the ratio of the two. The numbers in column 4 are, within the accuracy of the method, practically unity, showing that there has been no permanent alteration of specific resistance. With regard to the points *a*, *a* in each curve, which are out to the amount of 5 in 10,000, it must be remarked that these were the beginning of the second day's experiments. It was impossible to carry out a whole series of tests in one day, and, therefore, the wire was left over night with a weight of 14 lbs. on it.

The temporary alteration of specific resistance in the case of steel is considerably higher than the temporary or permanent alteration for copper or iron, as will be seen from the results given for these metals.

Tests of Copper Wire.

A very large number of tests were made on the effect of stretching on the specific resistance of copper wire. The wire was the same material as that used for tests of density, but of slightly smaller diameter. The following table gives the results of one set:—

(1.) Percentage stretching.	(2.) Ratio of weight specific resistance after weight was taken off to that before any weight was applied.
0·0000	1·00000
0·05	0·99969
0·5	0·99985
2·5	1·00104
6·5	1·00415
12·5	1·00666
16·2	1·00682
22·3	1·01083 (abnormal)

In this table the percentage stretchings are given instead of the weights producing the stretching, as, copper being so soft, the effect of the weights depends altogether on the times during which they were applied, and as these were very variable, a record of the weights would be of no value. For example, if a weight were put on, and allowed to hang for three minutes, then taken off, and a balance immediately found, and if the same weight be again put on, left for another three minutes, and a balance again found, it would be considerably different from before, showing that the wire had received an additional stretching. For this reason it is advisable to give the percentage stretching produced. In the case of steel wire it was quite different, as, throughout all the series, till the wire was just about to break, the elastic limit was not exceeded, so that the wire attained its ultimate stretching as soon as the weight was applied. With regard to this table, the results given are one set out of a great many which agreed very closely, $\frac{1}{10}$ per cent. being the greatest variations for corresponding stretchings; this set was chosen as having one peculiarity very well marked. This is with reference to the high value 1·01083, that is, an increase of 1 per cent. for a stretching of 22·3 per cent. The wire was almost at breaking point, and, on being examined, the surface was found to contain numerous cracks, many of which were so large that they could be easily felt by the fingers. These cracks were no doubt caused by the rapidity of stretching, as in other series in which the stretching was conducted more slowly they were not so apparent, and the change of specific resistance was not so great.

It was found quite impossible to smoothe out all the little deviations from perfect straightness in the copper wire. Although the first 14 lbs. were sufficient to keep it fairly straight, there were still little irregularities which could be seen with the eye. Not till about 56 lbs. were applied were these quite removed, and by that time there was also considerable stretching. The result was to make the apparent initial length shorter than it really was, and therefore the specific resistance seemed to diminish at first with the addition of weight. After the irregularities were taken out by stretching the length appeared to become greater, while the resistance did not increase, the result being that the specific resistance would seem to have diminished. This showed itself to a greater or less degree in all the series, according to the condition of the wire as to straightness. The values 0·99969, 0·99985 are clearly due to this. It might, therefore, have been more accurate to give the results, taking as the initial reading the lengths after the irregularities had been removed; but this would only very slightly alter the values, in fact by $\frac{1}{30}$ per cent.

Leaving out the value of the alteration just before breaking, we find in all our trials that the greatest increase of weight specific

resistance is not more than 0·7 per cent. This corroborates, for copper, very well what Lord Kelvin found in his first experiments on this subject. Since the density of the wire diminishes by 0·3 per cent., the increase of volume specific resistance is about 0·9 per cent., a result somewhat higher than Mr. Tomlinson's for the copper which he used, as he got 0·6 per cent. for the maximum value. We assume that the latter investigator means "volume specific resistance" by the term "specific resistance"; otherwise our results would agree more nearly with his. In any case, it seems quite certain that, in copper, the greatest alteration that can be produced by any mechanical treatment is not more than would be produced by a rise of temperature of 3° Centigrade.

For additional tests of copper, see end of paper.

Tests of Soft Iron Wire.

The wire, 0·8 mm. here used was exceedingly soft, and gave very satisfactory results. This metal is intermediate in properties between copper and steel. Whereas in copper the permanent alteration and stretching begin almost simultaneously with the application of weight, and in steel the alteration is almost altogether temporary, that is, only lasts while the weight is on the wire, in soft iron wire there is both a temporary and permanent alteration.

The same length of wire was taken as for copper and steel, and weights added by 7 lbs. each time, tests being made after each addition. A comparison was made of the ratio of the weight specific resistance with weight on to that before any weight was applied. This gives column 3 in the following table, and shows the ratio due to the sum of the temporary and permanent alterations.

Column 1.	Column 2.	Column 3.	Column 4.	Column 5.
Weight in lbs.	Percentage stretching.	Ratio of weight specific resistance with weight on to that before any weight was applied.	Ratio of weight specific resistance with weight on to that after the weight had been taken off.	Ratio of weight specific resistance after weight was taken off to that before any weight was applied.
14	0·00	1·00000	1·00000	1·00000
21	0·155	1·00056	1·00026	1·00030
28	0·21	1·00072	1·00103	0·99964
35	1·7	1·00128	1·00154	0·99969
42	4·0	1·00179	1·00160	1·00018
49	8·0	1·00424	1·00224	1·00192
56	15·5	no test	no test	1·00186

A comparison was also made of the ratio of the weight specific resistance while the weight was on with that immediately after the weight was taken off. The results are given in column 4 of the table. Column 5, which is obtained by dividing the results of column 3 by the corresponding numbers in column 4, gives therefore the permanent alteration due to stretching. In this table both the weights and the percentage stretchings are given, as it was found that with iron the difficulty experienced with copper of continuous stretching did not occur.

In order to test if there was any more permanent stretching after the wire had been allowed to rest for a week, another series of tests was made on the same wire at the end of the week, readings being taken on the application of every 7 lbs. as before. The results showed that the temporary alteration remained the same as before, being for 49 lbs. 1·002234, which, as will be seen by subtracting column 4 from column 2 to get the temporary alteration, the result being for 49 lbs. 1·00232, shows that the permanent alteration is practically constant after the first series of stretchings.

No tests have been made of alloys as yet, as it was thought more important to give as much time as possible to obtaining trustworthy results for pure metals.

The conclusions that have been arrived at from this investigation are that no mechanical treatment, such as stretching, drawing through holes in a steel plate, twisting, hammering, or combinations of these, all of which were tried, had any appreciable effect on the electrical properties of copper, iron, or steel.

The effect of annealing was also tried on a copper wire which had already been very much stretched. The wire was carefully heated to redness by means of a lamp all along its length, and a test then made. This was not found to bring its resistance back to its original value however.

As contrasted with the small effect that mechanical treatment has on the electrical properties of metals, it is interesting to notice the great influence even a trace of impurity in the metal has. For this purpose we include a table taken from Lord Kelvin's paper on "Analytical and Synthetical Attempts to ascertain the cause of the Differences of Electrical Conductivity discovered in Wires of nearly Pure Copper" (vol. 2, 'Math. and Phys. Papers'). This table (p. 299) gives an analysis, made by Professor Hofmann for Lord Kelvin, of several specimens of copper, and also their conducting powers.

From this table we see that an impurity of $\frac{1}{3}$ per cent. lowers the conducting power by as much as $5\frac{1}{2}$ per cent., and that the conducting power rapidly becomes enormously lower for increase in the impurities, 1·24 per cent. of the latter bringing down by 40 per cent. of its value when pure.

Conductivity of the wire in relative measure.	Qualitative analysis.	Percentage of copper.	Amount of impurities.
42·0	Copper, iron, nickel, arsenic, oxygen	98·76	1·24
71·3	Copper, iron, nickel, oxygen	99·20	0·8
84·7	Copper, iron, nickel (doubtful), oxygen	99·53	0·47
86·4	Copper, iron, nickel (doubtful), oxygen	99·57	0·43
102·0	Copper, iron, oxygen	99·9	0·10

Renewed Test on Copper Wire made after the Paper was written.

This series was made in order to find if, after the wire had been stretched almost to breaking and allowed to stand, there would be any more permanent alteration of specific resistance due to renewed application of weight. First of all, a series of stretchings was given to the wire, as in the former tests of the same wire. Column 2 of the following Table A was thus obtained, and will be found to agree very well with column 2 given under "Tests of Copper Wire" in the former series.

This wire was then tested by applying successive weights, the readings being taken with weight on and also after the weight had been taken off. Columns (2) and (3) of Table B give the results of these tests. Column (2) shows the ratio of the weight specific resistance with the weight on to that before the weights were applied for the second series. Column (3) gives the ratio with weight off to that before the weights were applied. Column (2) therefore shows the permanent and temporary alteration, and (3) shows the permanent alteration. There was, however, still a slight permanent stretching, which accounts for the small increases shown in Column (3). The results showed that when there was no permanent stretching application of weight only caused temporary alteration of weight specific resistance.

The numbers in columns (2) of Tables A and B when plotted on curved paper, with (for A) percentage stretchings as ordinates, and weight specific resistances as abscissæ, and (for B) weights applied as ordinates, give practically straight lines. This shows that the permanent alteration of specific resistance is directly proportional to the stretching, and the temporary alteration is directly proportional to the weight applied.

Table A.

(1.) Percentage stretching.	(2.) Ratio of weight specific resistance after stretching to that before stretching.
0·04	1·000000
0·04	1·000037
1·82	1·000711
4·7	1·002463
9·06	1·003776
16·63	1·007128
21·98	1·010363

Table B.

(1.) Weight applied in lbs. avoirdupois.	(2.) Ratio of weight specific resistance with weight *on* to that before applying weight.	(3.) Ratio of weight specific resistance with weight *off* to that before applying weight.
14	1·00000	1·00000
28	1·00487	1·00000
56	1·00131	1·00000
70	1·00185	1·00005
84	1·00237	1·00010
98	1·00283	1·00090

"The Action of Gravity upon *Bacterium Zopfii*."

By Rubert Boyce, M.B., M.R.C.S., Assistant Professor of Pathology, University College, London, and A. Ernest Evans, M.B., C.M., Glasgow. Communicated by Professor Victor Horsley, F.R.S. Received February 7,—Read February 23, 1893.*

(From the Pathological Laboratory of University College, London.)

[Plates 1 and 2.]

In May, 1892, Mr. Walter Spencer handed over to us the body of a cat in which he had discovered a double otitis media. Some of the pus was immediately inoculated upon gelatine, and two days later it was seen that the gelatine along the streak had liquefied, whilst the rest of the surface of the non-liquefied gelatine was covered by a

* Of the numerous photographs illustrating this paper, only five of the more typical ones are reproduced, namely, figs. 10, 12, 14, 23, 28. It has been thought better, however, to preserve the original numeration in the text, as the original photographs can be consulted if necessary.

regular growth of delicate white filaments. Re-inoculation proved the presence of more than one micro-organism, but we soon succeeded in isolating one which did not liquefy the gelatine, and which presented the exceedingly characteristic feather-like appearance well known to belong to *Bacterium Zopfii.* We were especially struck by the beautiful regular upward growth of the filaments upon the gelatine surface, for whether we placed the tubes with their mouth pointing upwards or downwards there appeared the same definite growth, provided the tubes were kept in a nearly vertical position. We accordingly immediately commenced an investigation, which we hoped might throw some light upon the meaning of this very definite symmetrical growth. With this end in view we first investigated the action of light, but whether we grew the bacterium in the light or in the dark, the symmetry remained the same. We now began to observe, however, that tubes which, after inoculation, were kept in a horizontal position exhibited an *irregular* growth. It therefore occurred to us to test the action of *gravity*, and we found in effect that revolving inoculated test-tubes slowly upon the clinostat* produced irregular growths, whilst *centrifugal force* induced, like gravity, a regular growth. *Bacterium Zopfii* was therefore negatively geotropic. These phenomena, viewed in the light of the more recent experiments upon the chemotactic and physiotactic properties of protoplasm—properties which appear already to throw very considerable light upon certain pathological problems—have led us to venture to record in detail our observations.

Bacterium Zopfii was accidentally discovered by Kurth† in the alimentary tract of the hen in 1883, and his description of the micro-organism, furnished from Zopf's laboratory, remains the standard one. In 1885 Professor Crookshank‡ obtained from the air in Johne's laboratory an organism which he named *Bacillus figurans* on account of its characteristically figured growth upon gelatine and agar; it proved to be identical with Kurth's bacillus. Lastly, as previously mentioned, we obtained our growth from the middle ear of the cat. These observations make it very probable that *Bacterium Zopfii* has a wide distribution, but whether it is pathogenic or not is still uncertain. We will, however, return to this point later on.

Bacterium Zopfii is pleomorphic ; it is a slender organism of about the same diameter as *Bacillus anthracis*, and its segments are of very variable length. It occurs in the dissociated motile state, the elements being actively movable coccal, bacteroid, bacillary, and

* We desire to express our thanks to Professor Oliver for not only the use of this and other instruments, but also for his friendly criticism and advice during the research.

† 'Bot. Zeitung,' 1883.

‡ 'Lancet,' 1885.

twisted forms; the degree of spirillation in the latter motile forms, however, appears only very slight, and we have not, as yet, encountered the pronounced spirilla seen in *Cladothrix.* It occurs in non-motile filaments of great length, which may be unsegmented or segmented. A very striking feature of these filaments is their proneness to twist, to weave themselves into striking patterns, and to form aggregating zoögleaform masses, of equally curious shapes. The segments of the filaments may occur in the form of cocci, bacteria, bacilli, vibrios, spirilla, and spirochætæ and in the various double forms of these. Kurth grew the organism in broth and on gelatine, and obtained in the former the motile and long filamentous forms, whilst the twisted varieties appeared wanting: this we likewise have noted. In gelatine he found that it caused an imperceptible amount of liquefaction and grew out in a radiate manner, and formed characteristic spirals. Crookshank grew it upon agar, and obtained a feather-like growth which he figures; this, however, we have always failed to obtain upon that medium. Kurth drew attention to the sensitiveness of the organism to changes in temperature, to the hindering action of the air, and to the great necessity for plenty of oxygen.

The Growth of Bacterium Zopfii *upon Gelatine.*

For our experiments we have always employed the 10 per cent. gelatine possessing a neutral or faintly alkaline reaction, but we have found no appreciable difference whether the organism was grown on gelatine with faintly alkaline or acidic reactions. We use the ordinary culture-tubes, Petri boxes, and a method of plate glass culture which we have found exceptionally useful. This method consists in placing a small piece of moist cotton wool in the bottom of a test-tube large enough to hold the ordinary 1×3 inch micro-slide; the test-tube is plugged, and the whole sterilised in the steam steriliser at 120° for 20 minutes. When cooled, a thin uniform layer of sterilised gelatine is very readily spread over the surface of the slide in the test-tube, which remains horizontal, by means of the balloon pipette of Pasteur: there is no fear of contamination. The test-tube is plugged and capped to prevent drying, and the slide is then always ready for inoculation. When a streak culture has been made upon the gelatine surface, and it is desired to examine the growth, the slide is removed and placed in slightly diluted spirit for a few hours, to fix the growth and extract the salts. The slide is then carefully dried, stained for a few seconds in gentian violet, washed with Gram's iodine, and nearly decolorised in spirit, again dried, mounted in Canada balsam, and covered with a sufficiently long cover-slip. By this method there is the least possible disturbance of the growth, and all our micro-

photographs have been taken from plates so prepared. The organism can always, however, be readily examined upon these gelatine slides without fixing or staining. The method of cover-slip impressions we have found not applicable in our case, as the growth is firmly embedded in the gelatine; it is useful, however, in the case of agar. For reasons which will be given later, it is necessary to observe under the microscope the ordinary test-tube growths *in situ* in the gelatine. For this purpose we rapidly warm the test-tube to just produce liquefaction of the surface of the gelatine in contact with the glass, and then slide it out upon a glass plate; a cover-slip is placed over the surface of the growth, and it can then be examined. Another method which we have employed, and one again which does not lead to any disturbance of the growth, is the well known method of drop cultures, but employing gelatine instead of a fluid medium.

Fig. 1 is a photograph of a 36 hours' growth upon gelatine, the result of a streak inoculation (see also fig. 12). The tube was kept vertical. It is the characteristic growth, *i.e.*, lines passing outwards and upwards at an angle of about 45°: the appearance is typically pinnate. In connexion with these vertical streak cultures we have noticed this very curious phenomenon, that no matter how crooked our original streak may be, yet the growth seems to form a new perfectly median axis for itself, from which the rami start; further, if instead of a vertical streak we make three or four streaks upon the surface of the gelatine at right angles to the long axis of the test-tube, the growth still tends to form a median line. These facts, coupled with our microscopic observations, pointed out to us that we might possibly be dealing with a branching phase similar to that met with in *Cladothrix dichotoma;* and, indeed, if we refer to Zopf's* drawings of *Cladothrix* we do recognize a pinnate arrangement of the so-called false branches of the filamentous organism. Billet† has termed this branched state the "filamentous," meaning thereby the "état d'accroissement proprement dit de la plante;" he has described it in *Cladothrix*, *Bacterium parasiticum*, *B. laminariæ*, *B. ureæ*, *B. Balbiani*, and *B. osteophilum*. In *Bacterium Zopfii* we shall regard the branching growths as corresponding to the "filamentous phase."

We soon found that we did not always obtain the characteristic growth, even in the test-tube; not only had the degree of sloping, as mentioned in the commencement, a considerable effect upon its regularity, but the temperature also exercises a marked effect. We obtained the best growths in a temperature of about 21° C. kept constant. In this way a perfect growth might be obtained in 24 hours, whilst if that portion of the test-tube which corresponded

* Schenk's 'Handbuch der Botanik.'

† 'Contrib. à l'Étude de la Morphologie et du Développement des Bactériacées.' Paris, 1890.

to the back of the gelatine was painted black, growth was still faster and might cover the surface in 12 hours. On the other hand we found accidentally that a reflecting surface retarded the growth. These latter phenomena we attributed to differences of radiation.

When next we directed our attention to the gelatine cultures on glass plates and in Petri boxes, we encountered a still greater number of irregularities. Upon the glass slides covered with a thin layer of gelatine we never obtained a good negatively geotropic growth; the usual condition was the irregularly branched appearance seen in figs. 15 and 16 under a low power. For the reasons of this we are still at a loss, unless it be the increased resistance of the gelatine. In the Petri boxes with a thickness of gelatine varying from $\frac{1}{4}$ to 1 inch, or even more, we obtained some instructive results. We never succeeded in getting symmetrical pinnate growths *on the surface* comparable to those in test-tubes; but that we obtained evidence of geotropism is manifest from the photographs, figs. 2, 3, 4, 5. In figs. 2 and 4 vertical streaks had been made; in fig. 3, a circular streak; and in fig. 5, a cross. The time occupied in the formation of these growths was relatively very much longer than in the case of test-tubes; moreover the filaments upon the surface rarely possessed the delicate character of that obtained in test-tube cultures; they had, in fact, as we shall subsequently see, the character of zoögleaform threads. In fig. 6 a horizontal streak has been made, and although the glass dish was kept vertical, an irregular growth has extended for an equal distance both above and *below* the horizontal line; *Bacterium Zopfii* may, in fact, readily extend downwards upon a surface, but it always does so in irregular masses. These results disappointed us at first, until on closer examination we found that in every case parallel threads *passed into the gelatine* almost invariably at the common angle of 45°. These geotropic threads had formed just as quickly as those found on the surface of the gelatine in the case of test-tube cultures; indeed, so rapid is their growth into the gelatine that they may extend a distance of $\frac{3}{4}$ to 1 inch in 12 hours. The symmetrical ingrowing into the gelatine we had also observed in the case of test-tube cultures where the growth upon the surface had been retarded by lowness of temperature, whilst painting the test-tube black in the manner previously referred to appeared to still further increase the inward growth. Fig. 7 is a view of the *back* of an early growth to show the regular ingrowing tendency; but photographs of the more perfect cases are impossible owing to the extreme delicacy of the filaments; fig. 5 is a later stage of 7. In fig. 6 the ingrowing is partially seen, and, as fast as the growth extended upon the surface, fresh rami projected symmetrically into the gelatine. We noticed in some cases that the filaments, after having penetrated the gelatine

and come in contact with the glass surface, might branch and be deflected back parallel to the ingrowing rami.

Growth upon Agar.—As stated above, we have never obtained a symmetrical growth in or upon this medium with or without glycerine. The growth is that figured in photograph 10. It takes place slowly, and the margins are irregular, being formed of scattered clumps of the bacterium.

Growth upon Potato.—Fig. 9 represents a potato culture; the faintly yellowish-white growth is observed to have no definite geotropic arrangement. The potato, it will be observed, appears black; this has been produced by means of iodine, a method which we have employed with success in demonstrating delicate growths upon potato surfaces.

Growth in Broth.—In broth abundant flocculi are formed.

Growth in Animals.—We have recorded how this micro-organism was first discovered by Kurth in the alimentary tract of the hen; whilst in our own case it was present in very great abundance in the pus of both the middle ears of the cat. It is just possible that in the latter (present) case the microbe gained access to the ear by the Eustachian tube, and therefore that the bacterium was originally in the mouth. It is conceivable, therefore, that it may, in common with the numerous other *Cladothrix* forms, be of more frequent occurrence in the mouth than is suspected. We have, however, made no observation upon this point. We inoculated two rabbits subcutaneously without result, and in another case the third of a test-tube full of a broth culture was injected into the peritoneum with like lack of success. These few experiments, however, prove little.

The Action of Oxygen and Carbonic Acid Gas upon Bacterium Zopfii.—We have already brought forward sufficient facts to show how sensitive this micro-organism is to changes of temperature. Fig. 11 represents a three days old growth in a test-tube kept in an atmosphere of carbonic acid gas; there is simply thickening of the original streak. Control test-tubes which were placed in oxygen at the same time exhibited well marked symmetrical growths from 12 to 24 hours. Further, if the tube in which the growth had been inhibited by the CO_2 were transferred to oxygen, a symmetrical growth occurred, as in fig. 12.

The Action of the Spectrum upon the Growth of Bacterium Zopfii.*—By means of a Zeiss sub-stage spectroscope a large spectrum was projected through the micro-photographic camera and focussed upon a Petri box of gelatine, across which a streak inoculation was made. From our results so far with gelatine or agar we can only say that

* For assistance in these experiments we are indebted to Dr. Fakirji Surveyor, who, with one of us, is engaged in the study of the action of the spectrum upon the pathogenic organisms.

the growth does not appear to be affected by any of the colours. Some of those who have seen our results think there is more growth towards the blue end; others, on the other hand, towards the red end. We ourselves have come to no definite conclusion as yet.

Microscopic Characters of the Cultures in the various Nutrient Media.—The series of photographs which illustrate these points have been fixed and stained in the gelatine, as previously mentioned. We have divided them into three groups. The first group, figs. 15 to 20, represent the *filamentous phase* of the early growth; fig. 17, for example, representing the long segments observed in the rapidly growing pinnate filaments. Groups II and III quickly follow upon the first. Group II, figs. 21 to 23, we venture to call the *skein phase*, which we think corresponds to the *état enchevêtré* of Billet. Group III, figs. 24—31, we think represent the *zoöglea phase.* There yet remains a fourth group, which we have not represented, and in which the segments live an independent existence (as partly shown in fig. 18) and very often are freely motile. We would call this the *dissociated phase*, the *état dissocié* of Billet. We have in this system of group phase division, therefore, followed Billet.* Whether this is a reasonable method of division will have to be judged from our photographs, from certain peculiarities exhibited by the above-mentioned groups, and lastly and chiefly from further examples.

In figs. 15 and 16, Zeiss obj., mm. 75 without eyepiece, the original streak of inoculation and the branches which spring from it are seen. The streak presents interesting peculiarities. Very slight liquefaction of the gelatine may be observed in it, and then very commonly swarming obtains; as previously mentioned, the motile segments may vary very greatly in length. In the figures in question clumps of bacilli occupy the central streak. In some cases, however, long zoöglea-like twisted masses, like those seen in fig. 25, run straight along this streak, it thus appearing as if their direction had been determined by the inoculating needle. In other cases the central streak is a mass of round zoögleaform collections. It will be seen that the micro-photographs in the first group afford very little evidence of the negatively geotropic tendency of the filaments, only the slight amount depicted in fig. 15 being often seen. The thicker branches in figs. 15 and 16, and all the branches in figs. 24 and 25, have assumed the zoögleaform condition. The microscopic picture presented by the rapidly growing pinnate form differs very markedly from these; in it nearly all the fibres in the field run parallel to one another, the microscopic picture then quite coinciding with the microscopic appearance seen, say, in fig. 1 or 14. In the pinnate forms it is exceedingly difficult to trace the origin of the fibres in the central line as well as to follow out the mode of branching; this is

* *Loc. cit.*

chiefly owing to the filaments slightly burrowing into the gelatine. The filaments may be very long and unbranched, but when branching does occur it appears to be brought about as in *Cladothrix* by the deflection of a segment to one side and subsequent growth. Soon after they are formed the long slender rami exhibit a remarkable proneness to twist and to segment, and then to pass into the zoögleaform or, less commonly, into the skein phases. The twisting is very striking, and some idea of it can be gathered from figs. 19, 25, 26, 27, 28, 29, 30. In none of these photos is the spirilliform twist of single filaments shown; it is, however, exceedingly common; the spiral is nevertheless seen in the zoöglea phases, figs. 25, 26, 30. There are, therefore, twists in the horizontal plane, and spirilliform twists. Concerning these twists it appears very remarkable that the majority appear to turn *inwards*, that is, in the opposite direction to the hands of the clock. To this point we will return when discussing the geotropism. It will be gathered from the above and from the photos that the segments may exhibit the "*form phases*" long ago laid stress upon by Zopf and Lankester; we need not, therefore, dwell upon them. The spiral is a striking feature in all gelatine preparations, but is only slightly marked in agar, whilst we have not as yet observed it in broth; but, we may add, we have not examined a sufficient number of these latter specimens.

Skein Phases.—The meaning which we wish to convey in this name is set forth in figs. 21, 22, and 23. We lay no stress upon it, but the term is convenient, for it is the form which can, we think, always be seen upon agar and stiff gelatine. It will be seen that it is quite unlike the zoögleaform phase. The filaments composing it are not held together by a common cementing substance, but are formed by plaits taking place in the course of a filament, as seen in figs. 21 and 19. Fig. 23 from an agar culture will at once recall the medusiform appearance of the anthrax growth. Its formation appears to be very closely related to the *stiffness* of the medium upon which it is growing, and appears always to occur *upon the surface of the medium;* the skeins are usually moist, and to the eye present a glistening frosted appearance.

Zoögleaform Phases.—This is the phase one commonly sees upon gelatine, and its striking appearance is well represented in figs. 24—31. Zoögleas are said to result from either the aggregation of dissociated elements or from the multiplication of segments which remain where they are formed, being held together by a common cementing substance. Of the former mode of formation we have no experience, and we venture to think that it is not common. If one turns to the descriptions of the higher bacteria, the impression is gained that the zoöglea masses have resulted from local multiplication; that this is so in *Bacterium Zopfii* the photographs show, as

well as the experiment of mixing a small quantity of the bacterium in liquefied sterile gelatine and then pouring it into a Petri dish; in the latter case numerous zöogleaform colonies develop in the solid gelatine, and send out branches which ramify in all directions. The branching zöoglea of *Cladothrix dichotoma*, commonly known as *Zöoglea ramigera*, follows closely the arrangement and branching of the filamentous stage; similarly, the "linear, globose, arborescent, reticular, and tesselated aggregations" of the *Bacterium rubescens* of Lankester probably represent local cell aggregation. As our figures show, the mode of aggregation leads to various forms, but they usually possess one feature in common, and that is spirillation, and a large number may be aptly compared to the twisted strands of a rope. The segments of which they are composed are small bacteroid or coccal forms, and they appear to be formed by the deflection and division of the minute segments into which the original single filaments break up. They rapidly follow the filamentous growth in the symmetrical pinnate cultivations; like the filaments which precede them, they assume the geotropic position. They may extend great lengths and be quite unbranched, or branching may occur as in figs. 30 and 31. Like the filaments, they form in the gelatine; we have not observed them in agar or broth. In fig. 31 a sarcina-like arrangement of the segments is seen; this is of interest, because, as Billet has pointed out, the sarcina of the lower bacteria probably is the representative of the zöoglea phase of the higher bacteria.

Upon the Phenomenon of Negative Geotropism exhibited by Bacterium Zopfii.

If a tube of gelatine is inoculated with a streak culture and kept in a nearly vertical position, a regular pinnate growth will, in the majority of cases, be obtained, identical with fig. 1. If a series of such tubes are placed at intermediate positions between the vertical and horizontal, it will be found that the growths become more and more irregular as the horizontal is reached. These results we have repeatedly obtained since May, 1892. But apparent failures do occur: *e.g.*, if the gelatine is too stiff, an irregular growth results, as in the case of agar, although the tube is kept vertical; microscopic examination of these irregular growths shows the skein phase. Similarly, if the temperature is low the symmetry is greatly interfered with. Experiments in the same manner with Petri boxes give, when sloped, the appearance already noticed in figs. 2 to 7. The pinnate growth is, in these cases, not so striking on the surface as with test-tube cultures, but, as we have previously remarked, the filaments which penetrate the gelatine ran a perfectly parallel course upwards, at an angle of about 45°, *into the substance of the gelatine;* the same

phenomenon may be observed in test-tubes, especially when the growth upon the surface tends to be irregular. If the Petri boxes are kept horizontal, the surface of the gelatine is soon covered with a quite uniform growth, in which, if rami can be distinguished, they take no particular direction. If, instead of these boxes, glass plate cultures are made, and in which, therefore, the coating of gelatine is necessarily thin, a pinnate growth with rami at the angle of about 45° is never obtained. The chance of getting a symmetrical growth upon a large flat surface is therefore much less than in the case of a test-tube. The explanation is difficult. In the test-tube, the surface of the gelatine has a slight curvature, and one often notices that towards the edge of the Petri dishes, where the gelatine is also slightly curved, the growth is more regular. Inequalities on the surface of the gelatine do cause alterations in the direction of the rami, and in some cases a few of the branches on this account appear to take a positive geotropic position. Thus, in addition to temperature and consistence of substratum, there are other circumstances which may favour or retard a symmetrical growth.

When test-tubes containing streak cultures are placed upon the vertical disc of the clinostat, and revolved at rates between one revolution in 2 minutes to 1 in 1 hour, there is in the first place a partial absence of the delicate rami seen in vertically sloped tubes, and when rami are formed they grow in various directions. Fig. 13 shows the irregular growth which first started, and grew over the middle area of the gelatine, and then the formation at the periphery of delicate rami, which in this case are completely *horizontal.* If a Petri dish is substituted for the test-tube, the result depicted in fig. 8 is obtained, a mode of growth which offers a striking comparison to figs. 2—6. Our results with the clinostat have been invariably the same, and, therefore, accord with what Sachs* obtained with the higher plants when the disc of the clinostat revolved slowly. In his case the direction of growth was neither that induced by gravity nor by centrifugal force. Having repeated these experiments very many times, we next tried Knight's experiment upon the action of centrifugal force upon growing plants. In his paper which was communicated to the Royal Society in 1806, he showed that, if young plants were rotated at rates varying between 80 to 250 times in a minute, that the ascending axis of the plant was centripetal, and the radicle centrifugal. Great was our satisfaction when we found likewise that the rami of *Bacterium Zopfii,* which, from the preceding experiments, we considered negatively geotropic, took a marked centripetal course when rotated in the horizontal at the rate of about 240 revolutions per minute. We obtained, in fact, as fig. 14 shows, much more symmetrical and beautiful results than

* Würzburg, Med.-Phys. Gesellschaft, March, 1872.

in the case of gravity. The angle remains about the same, and on microscopic examination the rami are found in nearly all cases strictly parallel and unbranched.

Our experiments thus show that we get a pinnate growth of *Bacterium Zopfii* when the surface of the medium is kept in or near the vertical, or when the culture is rapidly rotated in the horizontal. That an irregular growth obtains when the gelatine surface is horizontal, or when it is slowly rotated in the vertical. That it is necessary in order to demonstrate the upward growth to have a suitable temperature and suitable medium, and that other factors, the nature of which we do not understand, probably likewise influence the symmetrical growth. We would also draw attention to the fact that on vertical or nearly vertical gelatine surfaces, an irregular growth usually spreads *downwards* (compare fig. 6) upon the surface, and that when a horizontal surface of gelatine is inoculated, extension of the organism takes place vertically downwards into the substance of the gelatine; but in neither of these cases are the well-defined rami present. The question now remains, is the force which induces the upward growth of the protoplasmic threads of *Bacterium Zopfii, gravity?* Rozanoff* brought forward a beautiful example of upward growth in one of the Myxomycetes, *Æthalium septicum*. He grew it upon a nearly vertical surface of filter paper through which a very slow stream of water must have been constantly passing, as the upper edge of the paper rested in water. He found that the fungus grew upwards. He repeated Knight's centrifugal experiment, and found the fungus centripetal; during this experiment the filter paper was kept moist with water, and therefore the latter must have tended to fly outwards. He grew the *Æthalium* upon a horizontal surface of paper, *across* which a slow stream passed from one side to the other, and he found that growth did not take place more against the stream than with the stream. He argued whether this was an example of negative geotropism or of growth induced by the *contrary stream of water*, and he concluded that *Æthalium* was negatively geotropic, but that the *contrary flow of water* acted like the force of gravity and of centrifugalism. To our minds this does not seem an illogical conclusion, but Strasburger maintained that it was an example of *rheotropism* alone. Rheotropism, as far as fluid is concerned, cannot enter into our case. Our tubes were always plugged with absorbent cotton wool, and were not capped, and we never noticed moisture upon the surface of the gelatine, and, in any case, the geotropic rami tend to very slightly or deeply penetrate the gelatine. We have stated that both in vertical and centrifugalised gelatine growths, rami tend to pass *deeply into* the gelatine. Now we have observed, when we have placed in the

* 'Mém. Soc. Imp. des Sciences, Cherbourg,' vol. 14.

glass dishes gelatine to the depth of 1 inch or more, that at the lower portion of the streak of inoculation the rami which penetrated the gelatine were very distinctly more horizontal than those higher up. This phenomenon is also very conspicuously marked in the case of *centrifugalised tubes.* Further, our great difficulty in connexion with centrifugalising gelatine is to keep it in position, for in a large number of our experiments the gelatine is forced to the bottom of the tube, and the cotton plug is often driven some distance into the tube. We can see, further, that the compression to which the gelatine is subjected increases towards the distal end of the tube. Towards the distal end, however, the rami tend to become more horizontal, so it seems very much as if the tension of the gelatine caused the increased deviation to the horizontal. Having gone so far, we may ask why the rami are not orthogeotropic. We have many times called attention to the fact that the delicate filaments tend always to dip under the gelatine, and it may possibly be that it is the resistance of the gelatine which causes the deviation from the vertical to the curious angle of about 45°. We may here mention that this fact of the tendency to penetrate the gelatine is in apparent contradiction to the fact, that, as our experiments showed, CO_2 hinders and O favours symmetrical growth. Now, as a matter of fact, very little growth is obtained from a *stab* culture, so, therefore, it appears necessary that *some portion* of the growth should be close to or freely in contact with the air, and this obtains in *streak* cultures. To return to the action of the resistance of the gelatine, we have called attention to the fact that, next to the pinnate form of growth, the most striking feature in the life of *Bacterium Zopfii* is the tendency to curl. Kurth attributes this fact to the resistance of the gelatine. Now the twisting and the geotropic position of the rami we find tend to go hand in hand; they both tend to disappear upon agar or hard gelatine. Zopf noticed the absence of twisted forms in fluid. We find, as has been above stated, that the majority of the twists are in a direction contrary to the hands of a clock; many of the photographs show this, and these were taken at a time before we had directed our attention to the nature of the twist. Amongst the higher plants the inward twist is the commoner, and various theories have been made to account for the twisting. If we examine the rami which penetrate the gelatine we note that if they tend to be curved they tend to be convex on the lower surface, and that thus we tend to have two surfaces, a dorsal and a ventral, and can imagine with the botanist a difference in the tension of the protoplasm on these sides of the filament. According to De Vries, whose paper from the Wurtzburg Institute we have not been able to obtain, geotropism is not an improbable factor in the production of coils (Sachs). Wortmann* also

* "Theorie des Windens." 'Bot. Zeitung,' 1886.

regards coiling as the result of geotropism, and a second factor which he terms rotating mutation. In our case the coils may be related to the resistance of the gelatine and the negative geotropism. But spirillar forms are well known throughout the microphytes; they are present in *Cladothrix* and many more lowly-organised forms, and are found abundantly in fluid. Do, therefore, the spiral forms which we have described as forming in the gelatine correspond to the free spiral forms of the above examples? If they do, and we have not sufficient evidence to say they correspond, then the forces concerned in their formation in the one case would be equally applicable to the other.

DESCRIPTION OF PLATES.

PLATE 1. FIG. 10.—Represents a vertical streak inoculation upon agar. The irregular growth is characteristic.

FIG. 12.—This represents a culture which was first grown in an atmosphere of carbonic acid, and the only growth obtained was the thick irregular streak seen along the centre of the tube. It was then transferred to oxygen, and a pinnate growth obtained, as in the figure.

FIG. 14.—A typical symmetrical growth obtained by centrifugalising the culture tube in the horizontal.

PLATE 2. FIG. 23.—Skein, or Medusa-like growth, obtained upon agar.

FIG. 28.—Zooglea threads, showing the phenomenon of twisting. Gelatine growth.

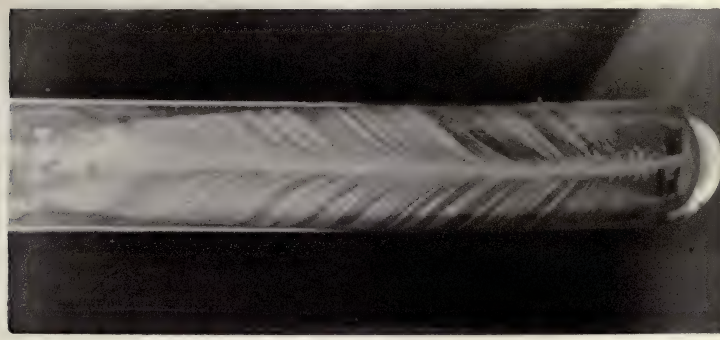


Fig. 14.

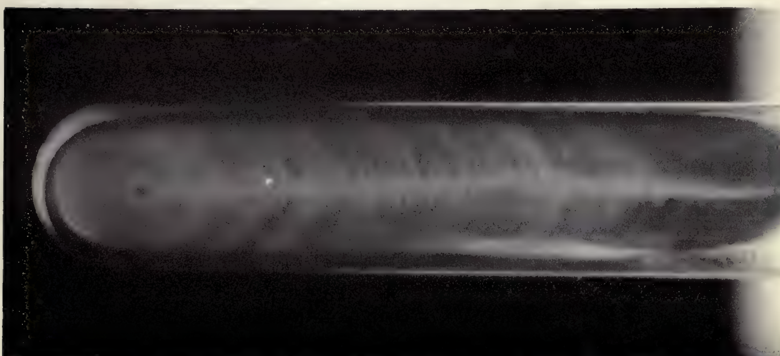


Fig. 12.

Fig. 10.

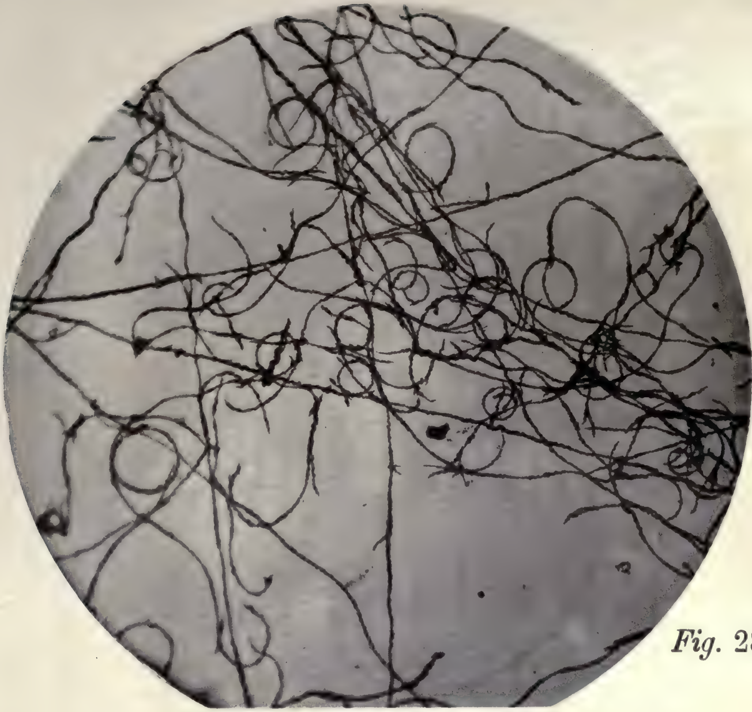


Fig. 28.

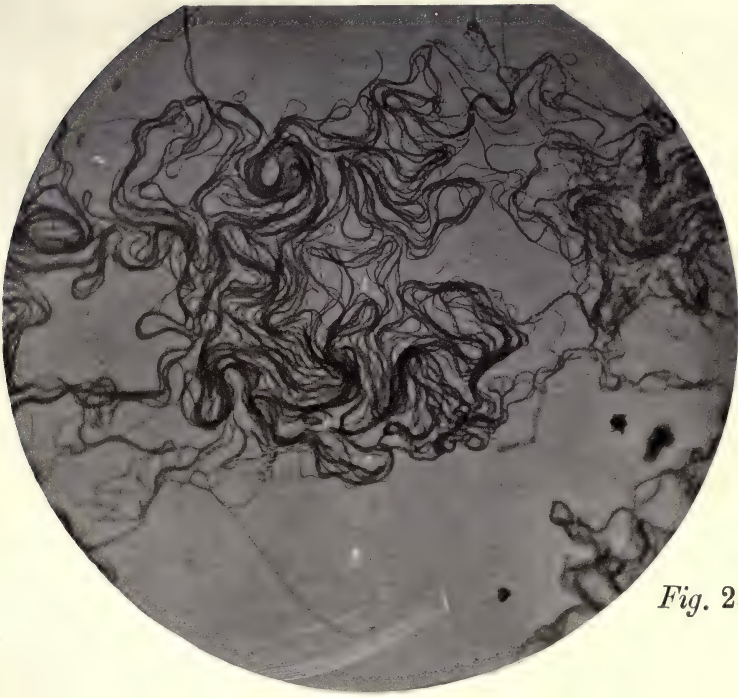


Fig. 23.

November 16, 1893.

Sir JOHN EVANS, K.C.B., D.C.L., LL.D., Vice-President and Treasurer, in the Chair.

Professor Arthur Mason Worthington was admitted into the Society.

A List of the Presents received was laid on the table, and thanks ordered for them.

In pursuance of the Statutes, notice of the ensuing Anniversary Meeting was given from the Chair.

Professor A. H. Church, Sir J. Cockle, and Professor W. C. Roberts-Austen were by ballot elected Auditors of the Treasurer's accounts on the part of the Society.

The following Papers were read :—

I. "On Hepatic Glycogenesis." By D. NOËL PATON, M.D., Superintendent of the Research Laboratory of the Royal College of Physicians of Edinburgh. Communicated by Professor MCKENDRICK, F.R.S. Received July 24, 1893.

(Abstract.)

Glycogenesis is here used in its original sense as indicating the whole process of sugar production in the liver.

The origin of sugar in the liver has been so conclusively demonstrated that it is not considered.

The evidence in regard to the relationship of hepatic sugar to hepatic glycogen is discussed, and is considered as conclusively in favour of Bernard's original view.

On the source of glycogen, all subsequent work has but established Bernard's conclusion : " L'acte *vital*, c'est la production du glycogène au sein du tissu vivant."

On the mode of conversion of glycogen to sugar, more recent work has not tended to confirm Bernard's view, that " L'acte *chimique* c'est la transformation du glycogène en sucre." The question of whether this conversion is due to a zymin, as held by Bernard, or to the vital action of the liver protoplasm, as suggested by recent writers, is, as yet, undecided.

The object of the present communication is to elucidate this point,

and the question considered is, how far the process of conversion of glycogen to sugar is dependent or independent of the life of the liver cells.

To determine this, the rate of conversion of glycogen to glucose in the excised liver, roughly minced, and kept at from 37° to 40° C. in normal salt solution, was first investigated. The object of the experiments not being to keep the liver cells alive as long as possible, but to differentiate between changes during the life of the cells and after their death, normal salt solution was used in preference to defibrinated blood.

Instead of estimating the sugar, as in Seegen's and Dalton's experiments, the glycogen was directly determined. For the analyses, the method of Brücke was found more suitable than that of Külz. The sources of error of the method are considered at length.

The following table gives a summary of the results of the experiments upon this subject, and shows that during the first half hour the conversion of glycogen is very rapid, that it steadily diminishes during the remainder of the first hour, and after this goes on very slowly.

No. of experiment.	Time in minutes from commencement.	Loss of glycogen per 100 parts liver.	Loss of glycogen per 100 parts liver per 10 minutes.
5, 1st hour	3	0·25	0·85
1 "	4	0·85	2·05
3 "	10	0·58	0·58
5 "	13	1·21	0·93
2 "	45	0·86	0·19
4 "	60	0·533	0·089
3, 2nd hour	95	0·5	0·05
4, 3rd hour	155	1·49	0·07
1, 4th hour	240	1·66	0·069
4, 5th hour	255	1·205	0·051
2, 6th hour	315	1·67	0·05
4 "	375	1·855	0·049

The relationship of the early rapid and the subsequent slow change of the glycogen to the condition of the liver cells is next considered, under these heads:—

A. Influence of Destruction of the Morphological Structure of the Liver Cells on Hepatic Amylolysis.

The liver of a rabbit, freshly killed and bled, was divided into three. One part, A, was pounded in a mortar with fine clean sand, and was then kept in salt solution at 40° C. The second part, B, roughly minced, was placed in a similar solution. The third part, C, was used to determine the initial amount of glycogen. At the end of some time A and B were boiled, and the glycogen extracted. The following two experiments show that destruction of the structure of the liver cells very greatly inhibits the conversion of glycogen.

A.	B.	C.	Time.
5·48	4·41	5·97	1 hr. 48 m.
5·061	2·336	5·267	4 8

B. Structural Changes in Liver Cells of Excised Liver kept under the Conditions above described.

The histological methods employed are described and the structure of the normal liver cell is considered. In the liver kept in salt solution at 40° C. as above described the protoplasm network becomes more apparent and then breaks up and tends to collect round the nucleus. The nucleus, somewhat later, loses its distinct outline and its network and stains diffusely. Finally, it breaks up.

These changes begin to manifest themselves usually within the first hour, and are often not completed even at the end of twenty-four hours.

The conversion of glycogen seems thus divisible into two periods.

1. An early period of rapid conversion occurring before obvious structural changes appear in the liver cells.
2. A late period of slow conversion after the changes above described have developed.

The rapid and extensive conversion appears to be inhibited by destroying the structure of the liver cells.

Further to elucidate the nature of these changes, the influence of various factors upon them was studied.

I. *Temperature.*—The possibility of distinguishing between zymins and living ferments by the influence of temperature of over 60° C. in destroying the latter, but not the former, is discussed, and it is concluded that it has a distinct, though restricted, value. So far as they go, the following experiments, showing that exposure for one hour to a temperature of 60° C. inhibits, but does not completely stop, hepatic amylolysis,* favour the view that the process is dependent on a living ferment rather than on a zymin.

* "Hepatic amylolysis" is used throughout this paper as an abbreviation fo "the conversion of hepatic glycogen to sugar."

Per cent. of Glycogen in Liver.

Initial glycogen.	Glycogen in part exposed to 60° C. and afterwards to 40° C.	Glycogen in part exposed from first to 40° C.
0·805 0·811	0·763 0·700	0·200 0·450

II. *Fluoride of sodium*, in 1 per cent. solution, according to Arthus and Huber, checks the action of protoplasm, but does not interfere with the activity of zymins. It is shown that it markedly retards hepatic amylolysis. It does not accelerate the structural changes in the liver cells.

III. *Chloroform water*, according to Salkowski, is a reagent which stops the vital action of protoplasms, but does not interfere with the action of zymins.

He gives an experiment upon its influence on hepatic amylolysis, from which he concludes that he has established the accuracy of Bernard's view, that the conversion is due to a zymin. In a series of experiments I have obtained results opposed to that of Salkowski. Three portions of the liver were taken. One (A) was used for the determination of the initial glycogen. The other two parts were placed in separate vessels in normal saline at from 37° to 40° C. Through one of these (B) a stream of chloroform vapour was conducted, a stream of air being directed through the other (C). The following results were obtained :—

A.	B.	C.	Time.
3·459	0·000	0·102	4 hrs.
0·811	0·165	0·450	5
Lost	0·735	1·012	3
2·160	0·841	1·273	4
2·07	0·73	0·97	6

Experiments were also made which show that the glycogen is actually converted to glucose.

Chloroform markedly increases hepatic amylolysis.

To determine whether the early rapid or the latter slow conversion is thus accelerated, the following experiment was performed. In it the large letters indicate the presence of chloroform, the small letters its absence :—

	Glucose.		Glycogen.		Time.
	Glucose per cent.	Gain per 10 min.	Glycogen per cent.	Loss per 10 min.	
Initial .	0·23	—	7·09		
B.....	1·39	0·308	5·68	0·31	45 min.
b	0·98	0·218	6·23	0·19	
C.....	1·96	0·06	5·00	0·07	next 90 min.
c	1·66	0·07	5·60	0·07	
D.....	2·58	0·036	4·60	0·02	next 180 min.
d	1·88	0·012	5·42	0·01	

It is the early rapid amylolysis which is accelerated.

The influence of chloroform upon the structural changes in the liver cells was investigated, and it was found that the disintegrative changes are markedly accelerated, occurring within the first half hour. This may indicate that the rapid early conversion is due to the more rapid katabolic changes in the protoplasm preceding its death.

The occurrence of glycæmia and glycosuria after the administration of chloroform rendered it desirable to ascertain if this accelerated amylolysis occurs in the living animal under the action of the drug. For this purpose, rabbits of one litter were kept on the same diet and daily weighed, in order to approximate the amount of glycogen in their livers. For each experiment a pair of as nearly equal weight as possible were selected. One was lightly anæsthetised for three or four hours, the other was not. The fallacies in this method of experiment are fully considered.

Animal.	Per cent. of glycogen in liver.	
	Check animal.	Chloroformed animal.
Rabbit............	1·437	0·75
,, ...	0·354	0·016
,, ...	3·91	1·664
,, ...	0·665	0·092
Dog..............	1·425	1·103
,, ...	1·000	

Such experiments seem to indicate that during life chloroform accelerates hepatic amylolysis.

IV. *Ether* is found to have the same action on the amylolysis in the excised liver as chloroform, but to a much less marked extent. Its action in bringing about structural changes in the liver cells is also less marked.

V. *Pyrogallic acid*, in 0·25 per cent. neutral solution, acts in the same manner upon the process of amylolysis and upon the liver cells.

VI. *Morphin* (0·005 to 0·025 per cent.), *curare*, *nitrate of amyl* (vapour through salt solution), *and salicylate of soda* (0·5 per cent.), neither increase hepatic amylolysis nor do they accelerate the cellular changes. The glycosuria caused by the administration of the first three of these is not due to increased hepatic amylolysis.

The products of hepatic amylolysis in the early and in the later stage were also investigated. In the early stage, glucose appears to be formed directly, and no intermediate bodies, such as dextrins or maltose, occur. In the later amylolysis, the former of these, possibly the latter, are always found.

In the light of these observations, the nature of the hepatic amylolysis is considered, and it is maintained that the evidence shows that the early rapid amylolysis is different from the later slow process, and that it is simply the result of the katabolic changes in the protoplasm accentuated as death occurs; that it is, in fact, simply an exaggeration of the process of amylolysis during life; and that it is in no way due to the action of a zymin, but is comparable to the production of mucin from mucinogen, and zymin from zymogen.

The later slow amylolysis is next considered. The development of an acid reaction, partly, at least, due to lactic acid, is shown; but experiments are given indicating that the acidity is not the cause of the amylolysis. The influence of micro-organisms is also investigated, and experiments are given to show that the process goes on as rapidly when these are excluded as when they are present. The evidence of the existence of a zymin in the dead liver is considered, and the conclusion is drawn that the later slow amylolysis is due to the action of such a zymin, probably developed during the disintegration of the liver cells.

II. "On certain Correlated Variations in *Carcinus mœnas*." By W. F. R. Weldon, M.A., F.R.S., Fellow of St. John's College, Cambridge, Professor of Zoology in University College, London. Received August 9, 1893.

In previous communications I have discussed the variations in size occurring in one or two organs of the common shrimp (*Crangon vulgaris*). In these papers it has been shown (1) that the observed

deviations from the average size of every organ measured are grouped symmetrically about the average, and occur wi.h a frequency corresponding closely to that indicated by the probability integral; and (2) that the "degree of correlation" between a given pair of organs is approximately the same in each of five local races of the species ('Roy. Soc. Proc.,' vol. 47, p. 445, and vol. 51, p. 2). In what follows I shall describe the results obtained by measuring certain parts of the shore crab (*Carcinus mœnas*) in two samples, one from the Bay of Naples, and one from Plymouth Sound, each sample consisting of 1,000 adult females.

1.—*The Variation of Individual Organs.*

The measurements made were as follows:—

1. *The total length of the carapace* (fig. 1, AB), in a straight line from the tip of the median inter-orbital tooth to the middle of the posterior margin.

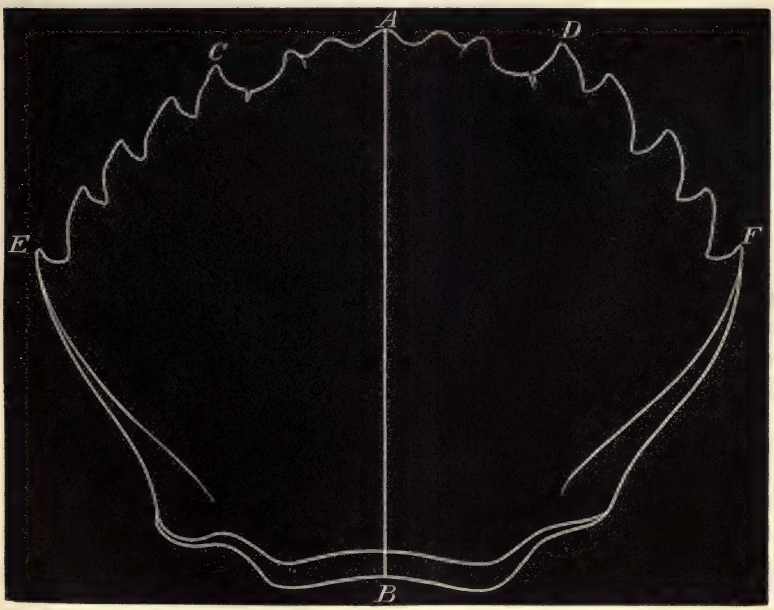


Fig. 1.—Diagram to show the parts of the carapace measured. The diagram is drawn to scale, the right half representing a perfectly average Plymouth crab, the left an average crab from Naples.

2. *The total breadth* of the carapace, in a straight line from tip to tip of the posterior lateral teeth (fig. 1, EF).

3. *The frontal breadth*, from tip to tip of the anterior lateral teeth (fig. 1, CD).

4. *The right antero-lateral margin*, from the tip of the median inter-orbital tooth to the tip of the postero-lateral tooth (fig. 1, AF).

5. *The right dentary margin*, measured in a straight line from the tip of the antero-lateral to the tip of the postero-lateral tooth (fig 1, DF).

6. *The left antero-lateral margin*, measured in the same way as the right.

7. *The left dentary margin.*

8. *The sternal breadth*, measured between the articulations of the great chelæ.

9. *The meropodite of the right chela*, measured in a straight line between the inner articulations.

10. *The carpopodite of the right chela*, from the inner articulation, in a straight line to the tip.

11. *The proximal portion of the same carpopodite*, in a straight line from the inner articulation to the tip of the inner spine, at the base of the dactylopodite.

The dimensions 2—11 were expressed in terms of the total length of the carapace taken as 1000; and, in order to reduce the effect of possible errors of measurement, the values so obtained were grouped together in fours, the groups being so selected that no two individuals in any one of them differed by more than 0·004 of the carapace length.

As an example of the way in which the values thus obtained were distributed, the measurements of the right antero-lateral margin in Naples and in Plymouth may be examined. The results of these measurements are shown in Tables I and II. The frequency with which every observed magnitude of this portion of the carapace occurred in the Naples specimens is given in the second column of Table I. The arithmetic mean of all these values is 752·22 thousandths of the carapace length; and the observations will be seen to cluster with a fair degree of symmetry around this value, the symmetry of distribution being, perhaps, more readily seen by the eye in the diagram, fig. 2. The total number of individuals in the sample was 999, and of these 513 had the antero-posterior margin greater than the average size, 486 having this portion of the carapace below the average. The arithmetic mean of all the deviations from the average, or "mean error" of distribution, was found to be 8·71 units; and the modulus is therefore $8{\cdot}71 \times 1{\cdot}77 = 15{\cdot}42$ units. A probability curve, with modulus $= 15{\cdot}42$ units, has been drawn by a dotted line in fig. 2; and the close agreement between this curve and the observed curve of distribution, which is indicated by a thick line, is very striking. In order to make a more accurate comparison possible, the number of individuals corresponding to each observed magnitude, on

the hypothesis that this probability curve represents the real distribution about the mean, has been calculated from the tables of the probability integral, and is given in the third column of Table I. In

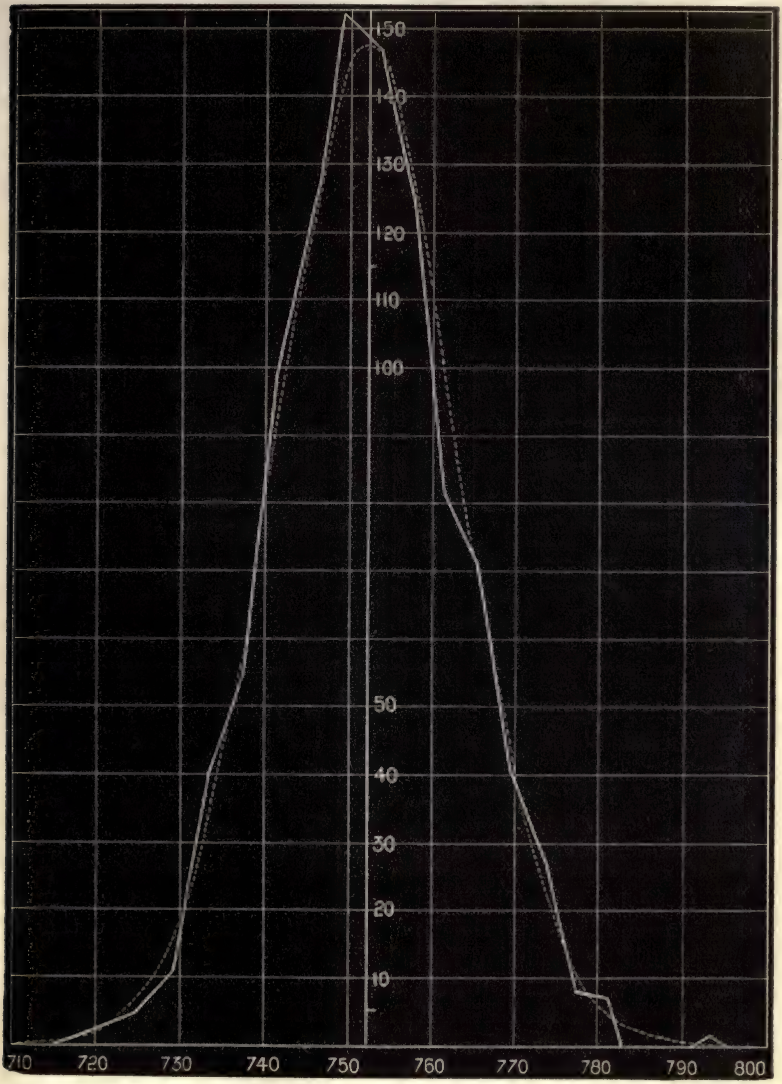


Fig. 2.—Diagram showing the frequency of occurrence of all observed lengths of the antero-lateral margin of the carapace in 999 female crabs from Naples. The abscissa scale represents thousandths of the total carapace length. The vertical scale represents numbers of individuals.

spite of some considerable discrepancies, the general agreement between the second and third columns of the table is undeniable.

Table I.—Distribution of Lengths of Antero-lateral Margin of Carapace in 999 Female *Carcinus mœnas* from Naples.

Dimension in thousandths of carapace length.	Number of individuals observed.	Number calculated from probability integral (c = 15·42).
792—795	2	
788—791	0	
784—787	0	16·4
780—783	7	
776—779	8	
772—775	28	22·2
768—771	41	42·1
764—767	72	70·2
760—763	82	102·0
756—759	126	130·0
752—755	147	144·8
748—751	152	141·1
744—747	121	120·3
740—743	98	87·0
736—739	55	59·6
732—735	40	34·3
728—731	11	16·7
724—727	5	
720—723	3	11·8
716—719	1	

The right antero-lateral margin of the Plymouth individuals, when treated in a similar way, gave the following results:—

Arithmetic mean	762·70	thousandths.
Mean error...........	9·77	,,
Modulus..............	17·29	,,

The frequency with which individual deviations from the average occur is compared with that indicated by a probability equation of the appropriate modulus in Table II.

These two examples will give a fair idea of the extent to which the distribution of the observed magnitudes of each organ about the mean of all of them corresponds to that indicated by the probability equation. A similar treatment of every other set of measures would serve no useful purpose; it will be sufficient to give, in the following table, the mean value, and the probable error of distribution about that value, of every organ measured. The probable error is given below, instead of the mean error, because it is the constant which has

Table II.—Distribution of lengths of Antero-lateral Margin of Carapace in 999 Female *Carcinus* from Plymouth.

Length in thousandths of carapace length.	Number of individuals observed.	Numbers indicated by probability integral.
796—799	6	
792—795	3	21·5
788—791	7	
784—787	19	23·2
780—783	44	45·2
776—779	70	61·6
772—775	94	87·5
768—771	101	110·5
764—767	140	125·6
760—763	128	134·0
756—759	105	112·7
752—755	100	97·2
748—751	79	62·2
744—747	47	53·4
740—743	25	29·2
736—739	20	15·8
732—735	4	
728—731	6	13·2
692—695	1	

the smallest numerical value of any in general use. This property renders the probable error more convenient than either the mean error, the modulus, or the error of mean squares, in the determination of the degree of correlation which will be described below.

Table III.—Mean Length and Probable Error of Various Parts of *Carcinus mœnas* in Naples and Plymouth, in thousandths of the Carapace length.

Organ.	Naples.		Plymouth.	
	Mean.	Probable error.	Mean.	Probable error.
Total breadth of carapace..	1261·98	13·44	1292·74	13·84
Frontal breadth of carapace	(see below)		604·94	9·96
R. antero-lateral margin...	752·22	7·26	762·70	8·24
L. " " ...	744·98	7·26	758·54	8·20
R. dentary margin	463·86	8·60	495·14	9·28
L. " "	457·74	9·32	491·86	9·44
Sternal breadth...........	434·66	6·44	442·30	6·52
Meropodite R. chela	262·38	5·88	279·74	5·36
Carpopodite R. chela......	597·22	21·68	608·90	15·32
Proximal portion R. chela..	395·46	22·80	414·38	14·00

The only case in which an undoubtedly asymmetrical result was obtained is that of the frontal breadth of the Naples specimens. From an inspection of the curve of distribution of these magnitudes, I was led to hope that the result obtained might arise from the presence, in the sample measured, of two races of individuals, clustered symmetrically about separate mean magnitudes. Professor Karl Pearson has been kind enough to test this supposition for me: he finds that the observed distribution corresponds fairly well with that resulting from the grouping of two series of individuals, one with a mean frontal breadth of 630·62 thousandths, and a probable error of 12·06 thousandths; the other with a mean breadth of 654·66 thousandths, and a probable error of 8·41 thousandths. Of the first race, Professor Pearson's calculation gives 414·5 individuals, of the second, 585·5. The degree of accuracy with which this hypothesis fits the observations may be gathered from fig. 3.

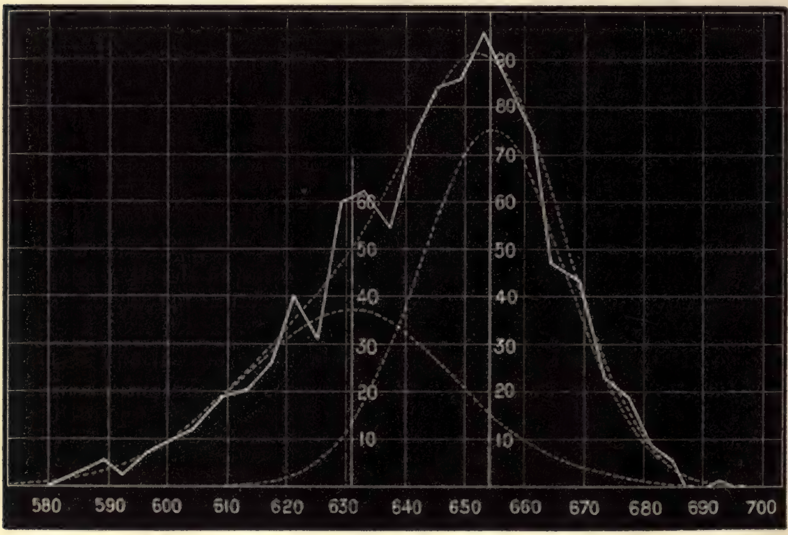


FIG. 3.—Diagram to show the distribution of all observed frontal breadths in the Naples specimens. The horizontal scale represents thousandths of the carapace length, the vertical scale numbers of individuals. Each ordinate of the upper dotted curve is the sum of the corresponding ordinates of the two component curves.

We may, therefore, assume that the female *Carcinus mœnas* is slightly dimorphic in Naples with respect to its frontal breadth; and that the individuals belonging to the two types are distributed in the proportion of nearly two to three.

2.—The Correlation of Pairs of Organs: Galton's Function.

The method adopted to determine the degree of correlation between two organs was that proposed by Mr. Galton ('Roy. Soc. Proc.,' vol. 40, p. 63). The measures obtained were sorted into groups, such that in each group the deviation X of an organ A from its average was constant. The mean deviation from its average of a second organ B was determined in each of these groups. Calling y_m the mean deviation of B, the ratio y_m/X was found to be approximately the same for all values of X. The same individuals were then sorted into groups in each of which the deviation Y of the organ B was constant: and in each of these groups, x_m, the mean deviation of A, was determined. As before, the ratio x_m/Y was approximately constant.

Mr. Galton has shown that if Q_a, Q_b be the probable errors of the organs A and B respectively, then

$$\frac{y_m/Q_b}{X/Q_a} = \frac{x_m/Q_a}{Y/Q_b} = r, \text{ a constant.}$$

The constant here denoted by r is evidently a measure of the degree to which abnormality in one organ is accompanied by abnormality in a second. It becomes ± 1 when a change in one organ involves an equal change in the other, and 0 when the two organs are quite independent. The importance of this constant in all attempts to deal with the problems of animal variation was first pointed out by Mr. Galton in the paper already referred to: and I would suggest that the constant whose changes he has investigated, and whose importance he has indicated, may fitly be known as "Galton's function."

As an example of the mode of determining this function, the correlation between the right and left antero-lateral margins of the Naples crabs may be examined, by means of the data given in the following tables.

In Table IV the measurements of the right antero-lateral margin have been sorted into groups, each group containing individuals which differ by not more than 0·004 of the carapace length, the magnitudes included in each group being given in the first column. In each of these groups the mean size of the left antero-lateral margin was determined, and the value obtained is given in the second column. From these data, and from the reciprocal data of the next table, the mean value of Galton's function was found to be 0·76, and the extent to which this value fits the individual cases may be estimated from the third and fourth columns of the table. In the third column, the value of the left antero-posterior margin corresponding to every value of the right margin

Table IV.—Mean Value of Left Antero-lateral Margin (*la*) for every Observed Value of the Right Antero-lateral Margin (*ra*) in 1000 Female *Carcinus* from Naples.

$M_{la} = 744{\cdot}98$; $Q_{la} = 7{\cdot}26$. $M_{ra} = 752{\cdot}22$; $Q_{ra} = 7{\cdot}26$.

Length of *ra*.	Mean associated length of *la*.	Length of *la* when $r = 0{\cdot}76$.	Difference.
Over 775	762·85	763·01	−0·16
772—775	761·66	761·15	+0·51
768—771	757·78	758·11	−0·33
764—767	754·78	755·07	−0·29
760—763	750·30	752·03	−1·73
756—759	749·26	748·99	+0·27
752—755	746·66	745·95	+0·71
748—751	742·50	742·91	−0·41
744—747	739·94	739·87	+0·07
740—743	737·66	736·83	+0·83
736—739	733·78	733·79	−0·01
732—735	729·58	730·75	−1·17
Under 732	729·50	726·19	+3·31

has been calculated on the assumption that $r = 0{\cdot}76$; and in the fourth column the difference between this value and that actually observed has been recorded. It will be admitted that these differences are in all cases small, being in every case less than four thousandths of the carapace length, that is to say, less than one unit of the measures employed.

The reciprocal result, obtained by sorting the individuals into groups in which the left antero-lateral margin had a constant value, and observing the mean associated value of the right margin, is shown in Table V, which will be understood without further explanation. In this table also the differences between the observed and the calculated values are small, being always less than 0·002 of the carapace length, or half a unit of measurement.

A similar examination of the right and left antero-lateral margins of the Plymouth specimens gave 0·78 as the mean value of Galton's function: and, considering the roughness of the method employed, and the small number of individuals examined, these two values may be considered identical.

The approach to identity between the corresponding values of this function in Naples and Plymouth is generally close, and may be gathered from Table VI.

The numbers given in this table show a remarkable degree of coincidence between the values of r derived from an investigation of the same pair of organs in the two races examined. There are, in some cases, considerable differences between the value in Naples and

Table V.—Mean Value of Right Antero-lateral Margin (*ra*) for every observed Value of Left Antero-lateral Margin (*la*) in 1000 Female *Carcinus* from Naples.

Length of *la*.	Mean associated length of *ra*.	Length of *ra* when $r = 0{\cdot}76$.	Difference.
Over 767	773·10	772·68	+0·42
764—767	767·22	767·82	−0·60
760—763	766·10	764·78	+1·32
756—759	761·90	761·74	+0·16
752—755	758·18	758·70	−0·52
748—751	755·26	755·66	−0·40
744—747	751·46	752·62	−1·16
740—743	749·46	749·58	−0·12
736—739	747·58	746·54	+1·04
732—735	744·90	743·50	+1·40
728—731	740·50	740·46	+0·04
724—727	737·06	737·42	−0·36
Under 724	734·78	734·38	+0·40

Table VI.—Values of Galton's Function for corresponding Pairs of Organs in 1000 Female *Carcinus* from Plymouth and 1000 from Naples.

Pairs of organs.	Naples.	Plymouth.
Total carapace breadth and frontal breadth.............	0·08	0·10
,, ,, R. antero-lateral margin	0·66	0·65
,, ,, L. ,, ,, ...	0·62	0·65
,, ,, R. dentary margin..........	0·50	0·55
,, ,, L. ,, ...	0·47	0·51
,, ,, sternal breadth.............	0·15	0·15
Breadth frontal and R. antero-lateral margin...........	0·29	0·24
,, ,, L. ,, ,, ...	0·27	0·22
,, ,, R. dentary margin................	−0·23	−0·18* (−0·24)
,, ,, L. ,, ...	−0·26	−0·20* (−0·24)
,, ,, sternal breadth..................	0·27	0·24
R. antero-lateral margin and L. antero-lateral margin....	0·76	0·78
,, ,, R. dentary margin.........	0·71	0·78
,, ,, L. ,, ...	0·60	0·70
,, ,, sternal breadth............	0·15	0·12
R. dentary margin and L. antero-lateral margin.........	0·63	0·67
,, ,, L. dentary margin..............	0·82	0·83
,, ,, sternal breadth.................	−0·03	−0·01
L. antero-lateral margin and L. dentary margin..	0·76	0·80
,, ,, sternal breadth............	0·15	0·18
L. dentary margin and sternal breadth.................	−0·04	+0·06
R. meropodite and R. carpopodite.....................	0·43	0·45
,, proximal portion of carpus	0·43	0·45

* These determinations contain one or two quite erratic values, by omitting which the result in brackets is obtained.

the value in Plymouth, as shown in the table; but these differences are in no case great enough to justify the assertion that the degree of correlation is really different in the two cases. As an example of the small importance of these differences, Table VII may be of use. In this table the mean values of the left dentary margin for every observed value of the right antero-lateral margin are given in the first two columns. The value of Galton's function for this pair of organs is given above as 0·70 in Plymouth, 0·60 in Naples. In the third column of Table VII, the mean value of the left dentary margin has been calculated for a value of $r = 0{\cdot}65$, and the results are compared with those obtained by observation.

Table VII.—Mean Value of Left Dentary Margin (*ld*) for every observed Value of the Right Antero-lateral Margin (*ra*) in 1000 Female *Carcinus* from Plymouth.

$M_{ld} = 491{\cdot}86$; $Q_{ld} = 9{\cdot}44$. $M_{ra} = 762{\cdot}70$; $Q_{ra} = 8{\cdot}24$.

Length of *ra*.	Mean associated length of *ld*.	Length of *ld* when $r = 0{\cdot}65$.	Difference.
Over 783	511·50	511·74	−0·24
780—783	507·50	505·87	+1·73
776—779	502·70	502·89	−0·19
772—775	499·38	499·91	−0·53
768—771	496·64	496·93	−0·29
764—767	495·02	493·95	+1·07
760—763	491·22	490·97	+0·25
756—759	486·98	487·99	−1·01
752—755	484·62	485·01	−0·39
748—751	483·18	482·03	+1·15
744—747	476·10	479·05	−2·95
740—743	474·06	476·07	−2·01
736—739	467·90	473·09	−4·81
Under 736	472·22	465·58	+6·64

With the exception of the last two values, the degree of coincidence between the calculated and observed values is close enough to be of value as an indication of the manner in which the variations are distributed: and even in the case of the last two values the difference is only 1·5 times the unit of measurement.

It may, therefore, be asserted that the investigation which has been described *does not demonstrate a difference* between the value of Galton's function for a given pair of organs in Naples and the corresponding value in Plymouth. The values obtained are not in all cases shown to be identical, but the differences between them are within the limits of error of the method employed; and in the worst case it has been shown that the errors arising from a neglect of the

observed discrepancy between two corresponding values are not of a very serious kind. So that in any discussion of the variation of the twenty-three pairs of organs discussed in the present paper, or of the pairs of shrimp organs discussed in my previous communication, it may be assumed as at least an empirical working rule that Galton's function has the same value in all local races. The question whether this empirical rule is rigidly true will have to be determined by fuller investigation, based on larger samples: but the value of a merely empirical expression for the relation between abnormality of one organ and that of another is very great. It cannot be too strongly urged that the problem of animal evolution is essentially a statistical problem: that before we can properly estimate the changes at present going on in a race or species we must know accurately (*a*) the percentage of animals which exhibit a given amount of abnormality with regard to a particular character; (*b*) the degree of abnormality of other organs which accompanies a given abnormality of one; (*c*) the difference between the death rate per cent. in animals of different degrees of abnormality with respect to any organ; (*d*) the abnormality of offspring in terms of the abnormality of parents, and *vice versâ*. These are all questions of arithmetic; and when we know the numerical answers to these questions for a number of species we shall know the direction and the rate of change in these species at the present day—a knowledge which is the only legitimate basis for speculations as to their past history and future fate.

III. "Contributions to the Mathematical Theory of Evolution." By Karl Pearson, M.A., Professor of Applied Mathematics, University College. Communicated by Professor Henrici, F.R.S. Received October 18, 1893.

(Abstract.)

1. If a series of measurements, physical, biological, anthropological, or economical, not of the same object, but of a group of objects of the same type or family, be made, and a curve be constructed by plotting up the number of times the measurements fall within a given small unit of range to the range, this curve may be termed a *frequency curve.* As a rule this frequency curve takes the well known form of the curve of errors, and such a curve may be termed a *normal frequency curve.* The latter curve is symmetrical about its maximum ordinate. Occasionally, however, frequency curves do not take the normal form, and are then generally, but not necessarily, asymmetrical. Such abnormal curves arise particularly in biological measurements; they have been found by Professor

Weldon, for the measurements of a certain organ in crabs, by Mr. Thompson, for prawns, by Mr. Bateson, for earwigs. They occur, however, in physics, *e.g.*, Dr. Venn's barometric and thermometric frequency curves; in anthropology, *e.g.*, Signor Perozzo's curves for Italian recruits, and Dr. C. Roberts' curves for the eyesight of Marlborough College boys and in fever mortality statistics; in economics, Mr. Edgeworth's curves of prices, and curves I have had drawn for rates of interest.

Frequency curves may, however, be abnormal and yet symmetrical. These are much more likely to deceive even the trained statistician; such curves might arise in target practice, and would be due, for example, to firing with equal precision, *but with a change of sighting* at mid-firing.

2. Abnormal frequency curves fall into three distinct classes:

a. Asymmetrical curves best represented by a point-binomial, or by its limit a continuous curve.

b. Asymmetrical curves which are the resultant of two or more normal curves, with different positions of axes, different areas, and different *standard deviations*—a term used in the memoir for what corresponds in frequency curves to the error of mean square.

c. Symmetrical abnormal curves, which are compounded of two or more normal curves having coincident axes but different areas and standard deviations, or of two normal curves with the same areas and standard deviations but different axes.

3. Let α be the area of any frequency curve, let the vertical through its centroid, or the line through its centroid perpendicular to the axis of measurement be drawn, and let the second, third, fourth, fifth, and sixth moments about this centroid-vertical, $\alpha\mu_2$, $\alpha\mu_4$, $\alpha\mu_5$, and $\alpha\mu_6$, be ascertained. This can be done by graphical or arithmetical processes indicated in the memoir, tables being given to assist the calculation in the latter case. Then we can treat the three classes of abnormal curves in the following manner:—

4. *Class a.*—Let the binomial corresponding to the curve be: $\alpha(p+q)^n$, where $p =$ probability in favour of an isolated event, $q =$ probability against, and $n =$ number of contributory "causes" in a single trial. For example: the simultaneous spinning of n teetotums with black and white sides proportional respectively to p and q, and α the total number of times the group of n is spun. Then it is easy to fit this point-binomial to a frequency curve of which the centroid vertical and μ_1, μ_2, μ_3, and μ_4 are known. The solution for this case is not discussed in the memoir, having been already dealt with by the author.

If it be desired to draw a continuous curve corresponding to the

asymmetrical curve, we can proceed as follows: Imagine n to be large, but the ratio q/p either small or large; then we can obtain *generalised form* of the normal curve of an asymmetrical character; its equation referred to the centroid vertical is

$$y = \frac{a}{\sqrt{(2\pi\mu_2)}}\left\{\frac{\sqrt{(2\pi\beta)}\,\beta^\beta e^{-\beta})}{\Gamma(\beta+1)}\right\}\left(1+\frac{\mu_3}{2\mu_2^2}x\right)^{\beta-1} e^{-\frac{2\mu_2}{\mu_3}x},$$

where β stands for $4\mu_2^3/\mu_3^2$ and $\Gamma(\beta)$ is the Eulerian gamma function.* Putting $\mu_3 = 0$ for an asymmetrical curve, the equation takes an indeterminate form obtained by putting $\beta = \infty$, but on evaluation we have the usual normal form:

$$y = \frac{\alpha}{\sqrt{(2\pi\mu_2)}} e^{-\frac{x^2}{2\mu_2}}.$$

This generalised probability curve fits with a high degree of accuracy a number of measurements and observations hitherto not reduced to theoretical treatment, *e.g.*, barometric frequency curves.

The importance of this first dissection of asymmetrical frequency curves lies in the fact that it measures the theoretical number n of contributory "causes" and the odds $p:q$ that an element of deviation will be *positive.* The whole theory is, however, of an elementary character, and, as biological frequency curves often tend to develop a double-humped character,† they do not invariably fall under this class, and it is not dealt with at length in the memoir.

5. *Class b.*—The general theory of the dissection of a given abnormal frequency curve into m components is not dealt with, partly on account of its exceedingly great analytical difficulties, partly because there is an *a priori* probability that we have a mixture of only two homogeneous groups, or from the standpoint of evolution that the species will break up at first into two, rather than three or more, families. At any rate, the dissection into two is likely to give us either the chief components or a measure of the chief asymmetry of the curve. Supposing the curve asymmetrical, it is shown that the solution of the problem is theoretically unique, but it is pointed out that in statistical practice our curve is based upon a limited number of measurements, and is therefore not an accurately true compound of two normal groups. A theoretical test is given to distinguish between the better of two or more solutions. The method adopted for the dissection is based on equality of the first five moments and of the areas of the abnormal curve and of its two components. This method is justified in the same manner as the determination of the

* If β be large, it may be taken as approximately whole, and the factor in round brackets is then unity.

† *E.g.*, claspers of earwigs, height of Italian recruits of various special provinces, short sight of Marlborough boys, height of inhabitants of Doubs, &c.

normal curve by fitting any series of observations by aid of the area and the first two moments (*i.e.*, the first moment gives the mean, and the second the error of mean square) is justified. The method leads to what is termed the *fundamental nonic*, every root of which gives a real or imaginary solution of the problem. The best solution is selected by the criterion that it gives the closest approach to the given frequency curve in the value of the sixth moment. From the nonic is deduced a quadratic for the areas of the components corresponding to each solution. If both roots of this quadratic are real and positive, we have either a mixture of two heterogeneous species, or evolution is breaking the homogeneous material up into two families of different magnitudes, different means, and different standard deviations from the mean. If one root of the quadratic be real and positive, and the other real and negative, we have evolution destroying a certain percentage round a certain mean out of an initially homogeneous and normal group.

Should one of the standard deviations be imaginary, we get the percentage of anomalous and irregular measurements in a homogeneous group.

6. *Class c.*—The solution here is unique and depends upon the equality of the areas and of the first *six* moments; for all odd moments vanish, and we have four quantities to determine, *i.e.*, the percentages of each group and their standard deviations. The solution depends on a quadratic for the areas, and the same remarks apply as to the quadratic for Class *b*.

7. Rules are given for detecting whether we have a mixture of two groups, or whether a differentiation into species of a homogeneous material is going on; and also rules for measuring the amount of asymmetry which is to be considered significant. The former rules are, briefly:—

i. Select the most asymmetrical curve out of the curves for the organs measured; dissect it into two curves or groups by the method for Class *b*.

ii. Select the most symmetrical curve out of the curves for the organs measured and dissect it into two groups by the method for Class *c*, or, if it have significant asymmetry by the method for Class *b* again. Then (α), if the first dissection is possible and the second is not, a real evolution is going on; (β), if the first dissection is possible and the second is possible, and both groups give sensibly the same percentages, we have a mixture of two heterogeneous materials and no true evolution, unless the organs be so closely allied that one must vary directly with the other (*e.g.*, length of right and left legs); (γ), both dissections are possible, but give groups with different percentages; we have *both* organs evolving differently at the same time.

8. The theory is applied to Professor Weldon's measurements on Naples crabs. It is shown that his material is absolutely homogeneous, all roots of the nonic for No. 4 organ leading to imaginary solutions, even its real root. On the other hand, it is shown that the Naples crabs are breaking up into two different sized families, owing to evolution in their foreheads. The theory is further applied to Mr. Thompson's measurements of the carapace of prawns (1,000 measurements). It is shown that we have in the measurements a very small percentage of anomalous results, corresponding to prawns deformed in this organ, or that there is, on the other hand, a small but unstable giant population mixed with the normal population. Which of these results is to be considered the true answer to the problem can only be determined after an analysis of the frequency curves for other organs.

From the mathematical standpoint, the memoir illustrates the determination of the roots of equations of the ninth degree, and the calculation of the higher moments of curves.

IV. "Experiments in Heliotropism." By G. J. ROMANES, F.R.S.

Received October 2, 1893.

I cannot find in the literature of heliotropism that any experiments have hitherto been made on the effects of interrupted illumination, when the periods of illumination are rendered as brief as possible—*i.e.*, instantaneous flashes of light. Accordingly I have conducted an extensive research on heliotropism, where the flashes have been caused either by means of electric sparks in a dark room, or by the opening of a photographic shutter placed before the plants in a camera obscura with an arc light or Swan burner, at a distance of several feet on the other side of the shutter. The electric sparks were made either with a Wimshurst machine, induction sparks, or by means of the following contrivance. From the binding screws of the condenser of a large induction coil copper wires were led to a cup of mercury, where, by means of an electro-magnet suitably actuated by clock work, a current was closed and opened at any desired intervals: each break was therefore accompanied by a brilliant spark. A thick plate of glass was interposed between the seedlings and the electrical apparatus. In all the experiments here described the plants employed were mustard seedlings (*Sinapis nigra*), previously grown in the dark until they had reached a height of between 1 and 2 inches. Save when the contrary is stated, in all the experiments comparative estimates were formed by using the same pot of seedlings: during the first half of a comparative experiment half of the seedlings were protected from the light by a cap of cardboard covering half the pot;

during the second half of the experiment this cap was removed, and the pot turned round, so as to expose the previously protected seedlings to the influence of the light. The principal results thus obtained, and frequently corroborated, were as follows.

I. Even having regard to the fact that for equal strengths of a stimulus excitable tissues are more responsive in proportion to the suddenness of the stimulus (or in a kind of inverse proportion to the duration of the stimulus), the heliotropic effects of such flashing stimulation as is above described proved to be much greater than might have been antecedently expected. This was shown to be the case whether the effects were estimated by the rapidity with which the seedlings began to bend after the flashing stimulation was begun, or by that with which they continued to bend until attaining a horizontal line of growth, *i.e.*, bending to a right angle. Thus, at a temperature of 70° Fahr., and in a moist camera, vigorously growing seedlings begin to bend towards the electric sparks ten minutes after the latter begin to pass, and will bend through 45° in as many minutes; frequently they bend through another 45° in as many minutes more. This is a more rapid rate of bending than can be produced in the same pot of seedlings when the previously protected side is uncovered and exposed for similar durations of time, either to constant sunlight, or to constant diffused daylight. This is the case even if the sparks (or flashes) succeed one another at intervals of only two seconds.

II. It would thus appear that the heliotropic influence of electric sparks (or flashes) is greater than can be produced by any other source of illumination. But, in order to test this point more conclusively, I tried the experiment of exposing one half-pot of seedlings in one camera to the constant light of a Swan burner, and another half-pot of similar seedlings, in another camera, placed at the same distance from the same source of light, but provided with a flash shutter working at the rate of two seconds intervals. The amount of bending in similar times having been noted, the pots were then exchanged, and their previously protected halves exposed to the constant and the flashing light respectively. In both cases, the rapidity with which the bending commenced, and the extent to which it proceeded in a given time after commencement, were considerably greater in the seedlings exposed to the flashing than to the constant source stimulation. The same is true if, instead of a Swan burner, the source of light is the sun.

III. Many experiments were tried, in order to ascertain the smallest number of sparks in a given time which would produce any perceptible bending. Of course the results of such experiments varied to some extent with the condition of the seedlings. But in most cases, with vigorous young mustard seedlings and careful observation,

bending could be proved to occur within fifteen to thirty minutes, if bright sparks were supplied at the rate of only one per minute. The most extreme sensitiveness that I have observed in these experiments was that of perceptible bending after half-an-hour's exposure to electrical sparks following one another at the rate of fifty in an hour. This result would appear to indicate that in heliotropism under flashing light there need be no summation or "staircase effect"; but that each flash or spark may produce its own effect independently of its predecessors or successors.

IV. It is noteworthy that, while the heliotropic effects of flashing light are thus so remarkable, they are unattended with the formation of any particle of chlorophyll. In the many hundred pots, and therefore many thousands of plants, which have passed under my observation in this research I have never seen the slightest shade of green tingeing the etiolated seedlings which had bent towards flashing light. On one occasion I kept a stream of 100 sparks per second illuminating some mustard seedlings continuously for forty-eight hours; and although this experiment was made for the express purpose of ascertaining whether any chlorophyll would be formed under the most suitable conditions by means of flashing light, no change of colour in any of the seedlings was produced.

With the exception of those mentioned in the last paragraph, all these results were obtained by using sparks from the coil condenser, as above explained. These sparks were very brilliant, and yielded the maximal results, which alone are here recorded.

V. "Experiments in Germination." By G. J. Romanes, F.R.S.

Received October 2, 1893.

The primary object of these experiments was to ascertain whether the power of germination continues in dry seeds after the greatest possible precautions have been taken to prevent any ordinary processes of respiration for practically any length of time.

The method adopted was to seal various kinds of seeds in vacuum tubes of high exhaustion, and after they had been exposed to the vacuum for a period of fifteen months to remove them from the tubes and sow them in flower-pots buried in moist soil. In other cases, after the seeds had been *in vacuo* for a period of three months, they were transferred to sundry other tubes respectively charged with atmospheres of sundry pure gases or vapours (at the pressure of the air at time of sealing); after a further period of twelve months these sundry tubes were broken, and their contents sown as in previous case. In all cases, excepting that of the clover, the seeds sown were weighed individually in chemical balances, and seeds of

Kinds of seed.	Air (control).	Vacuum.	Oxygen.	Hydrogen.	Nitrogen.	Carbon monoxide.	Sulphuretted hydrogen.	Aqueous vapour.	Ether.	Chloroform.	Totals.
Mustard	6	1	8	6	1	1	6	1	8	8	46
Red beet	14	1	14	16	17	12	19	10	17	13	133
Clover	4	3	1	5	2	1	4	0	1	0	21
Peas........	6	1	8	9	8	6	6	9	9	0	62
Beans	2	2	2	2	2	2	0	2	2	2	18
Spinach	4	3	3	4	3	4	1	4	3	2	31
Cress	9	8	10	10	10	6	8	0	4	5	70
Barley	2	4	4	4	4	1	0	4	6	3	32
Radish	4	8	1	1	9	1	0	6	4	8	42
Totals........	51	31	51	57	56	34	44	36	54	41	455

similar weights taken from the same original packets were similarly sown as controls.

The table on p. 336 gives the results of one such series of experiments, where the exhaustion of the tubes was kindly undertaken by Mr. Crookes, F.R.S., to whom I must express my best thanks for the assistance he has given. But it may be mentioned that other series of experiments yielded virtually the same results.

With the exception of the beans, where only two were sown, ten weighed seeds were sown out of each of the tubes, and also out of each of the control packets which had been kept in ordinary air from the first. These results amply prove that neither a vacuum of one-millionth of an atmosphere, nor the atmospheres of any of the gases and vapours named in the above list, exercised much, if any, effect on the germinating power of any of these seeds. I may add that the same remark applies to an atmosphere of carbon dioxide, although in the particular series of experiments quoted this gas was accidentally omitted.

A subsidiary object of these experiments was to ascertain whether any appreciable variations would be caused in plants grown from seeds which, before germination, had been submittted to the conditions above explained. Hundreds of plants of the kinds named in the above table were grown from the seeds in the various tubes. But in no one instance was there the smallest deviation in any respect from the standard type grown from the corresponding control packet.

It will be observed that, in the case of the beet-root, a larger number of plants were developed in many of the pots than the ten seeds which had been sown in each. This I found to be due to the fact that beet-root seeds very frequently throw up two seedlings apiece. Not so frequently, but still very often, they yield three, and sometimes even four.

Further experiments are in progress.

Presents, November 16, 1893.

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The Syndics of the Cambridge University Press.

Bronze copy of the Medal struck by the Royal Society in honour of Capt. Cook, F.R.S. By Sir J. Evans, Treas. R.S.

Framed Drawing of H.M.S. "Resolution," commanded by Capt. Cook, F.R.S., during his Second Expedition to the South Seas.
Mr. H. Rickinson.

Bust, in plaster, of the late Dr. J. P. Joule, F.R.S. Mr. B. Joule.

Four Lantern Slides, Eclipse of April, 1893, from Prof. Schaeberle's Negatives.
Lick Observatory, per the British Astronomical Association.

November 23, 1893.

Sir JOHN EVANS, K.C.B., D.C.L., LL.D., Vice-President and Treasurer, in the Chair.

A List of the Presents received was laid on the table, and thanks ordered for them.

In pursuance of the Statutes, notice of the ensuing Anniversary Meeting was given from the Chair, and the list of Officers and Council nominated for election was read as follows:—

President.—The Lord Kelvin, D.C.L., LL.D.

Treasurer.—Sir John Evans, K.C.B., D.C.L., LL.D.

Secretaries.— { Professor Michael Foster, M.A., M.D.
The Lord Rayleigh, M.A., D.C.L.

Foreign Secretary.—Sir Joseph Lister, Bart., F.R.C.S.

Other Members of the Council.—Professor Isaac Bayley Balfour, M.A.; Andrew Ainslie Common, LL.D.; Andrew Russell Forsyth, Sc.D.; Richard Tetley Glazebrook, M.A.; Professor Alexander Henry Green, M.A.; Sir John Kirk, K.C.B.; Professor Oliver Joseph Lodge, D.Sc.; Sir John Lubbock, Bart., D.C.L.; William Davidson

Niven, M.A.; William Henry Perkin, LL.D.; the Marquis of Salisbury, K.G., M.A.; Professor J. S. Burdon Sanderson, M.D.; Adam Sedgwick, M.A.; Professor Thomas Edward Thorpe, Sc.D.; Professor William Augustus Tilden, D.Sc.; Professor W. Cawthorne Unwin, B.Sc.

The following Papers were read:—

I. "The Photographic Spectrum of Electrolytic Iron." By J. Norman Lockyer, F.R.S. Received October 27, 1893.

(Abstract.)

This investigation is an extension of the author's earlier researches on the spectrum of iron, and supplements the important paper published by Thalén in 1884. Since the work was commenced, two other papers on the spectrum of iron have been published, one by Professors Kayser and Runge, of the Hanover Technical High School, and the other by Mr. F. McClean. In the former, the arc spectrum of ordinary commercial iron was in question. The wave-lengths were determined by micrometric measures, a number of standard lines being used to construct the interpolation curves. Mr. McClean's paper consists of a series of photographic comparisons of the spark spectrum of iron and the solar spectrum.

A knowledge of the true spectrum of iron is of the utmost importance, both for solar and stellar work. Comparisons in terms of iron are very important, and such a comparison is a natural first step in their study when we have a terrestrial iron spectrum about which there is no doubt.

Thalén's memoir is practically complete so far as the visible arc spectrum of iron is concerned. The photographic arc spectrum over the same region has not, however, hitherto received such minute attention. This subject has been taken up at Kensington by photographically comparing the spectrum of iron with the solar spectrum, between Fraunhofer's lines K and C. For this purpose, a small portion of some iron of exceptional purity, prepared by electrolytic deposition by Professor W. C. Roberts-Austen, was placed by him at the author's disposal. A part was arranged to form the poles of an electric arc lamp, which was placed about 4 ft. from the slit of a Steinheil spectroscope, having three prisms of 45°, and one of 60°, an image of the arc being formed on the slit plate by a suitable lens. The current employed was from a "Victoria" Brush dynamo, driven by an "Otto" gas engine, and making about 850 revolutions per minute.

The region of the spectrum between K and C was photographed on four plates. For the region between K and F ordinary photo-

graphic plates were used, but for the spectrum from F to C specially prepared plates were employed.

The wave-lengths of the lines have been determined by reference to Professor Rowland's second series of photographic prints of the solar spectrum, and, consequently, are all expressed on Rowland's scale. Tables of the wave-lengths and intensities of all the lines in the photographs have been compiled, and a comparison instituted between the Kensington results and those of Thalén, Messrs. Kayser and Runge, and McClean. Though the iron used was the purest obtainable, a few of the lines are probably due to impurities, and these have been indicated in the tables.

Many of the faint lines mapped by Messrs. Kayser and Runge are evidently due to impurities in the iron used in their researches, and the origin of these lines has been traced, as far as possible, by reference to the, as yet, unpublished Kensington maps of metallic arc spectra, and the results are shown in the tables.

The comparisons have led to the following conclusions:—

1. The visual spectrum, as mapped by Thalén, differs but slightly in essential points from that which has been photographed at Kensington. The principal difference is the greater number of lines mapped by Thalén in all regions except that between wave-lengths 4000 and 4300. This is probably to be accounted for by the insufficient exposure of the photographs, which was necessitated by the limited amount of material available for the experiments.
2. The comparison with the spectrum photographed by Mr. McClean, indicates that the experimental conditions employed by him produced a temperature not greatly differing from that of the arc employed at Kensington. There are only a few lines which are not common to the two series of photographs, and these, in many cases, can, with certainty, be ascribed to impurities present in one case and not in the other. The apparent differences in intensity between some of the lines which are common to both are mostly due to superposition of the spectrum of air upon that of iron in Mr. McClean's photographs. In some cases, however, there appears to be a real difference in the intensities of the lines, and this may probably be ascribed to the difference between the temperatures employed.
3. The number of lines mapped by Messrs. Kayser and Runge is considerably in excess of that mapped by the author in corresponding regions of the spectrum. The comparison indicates that this is partly due to the fact that the iron employed in their experiments contained a greater number of impurities than that employed at Kensington. No origins have been

traced for many of the lines present in their photographs, which do not appear in the Kensington photographs, and some of these may, therefore, be really due to iron, their absence from the Kensington photographs being due to insufficient exposure, or to the employment of different temperatures. The almost constant difference of 0·1 tenth-metre between the two sets of measures is a satisfactory indication of the accuracy of both.

4. The impurities which contribute the greater number of foreign lines to the spectrum of the electrolytic iron employed by the author are Ca and Mn, though there is decided evidence of the presence in minute quantities of various other elements.

This research on the arc spectrum of iron is made in connection with a wider investigation on the arc spectra of the other metallic elements, the results of which will be communicated to the Society in due course.

II. "Magnetic Observations in Senegambia." By T. E. Thorpe, F.R.S., and P. L. Gray, B.Sc., Assoc. R.C.S. Received October 31, 1893.

On the occasion of the recent Eclipse Expedition to Senegambia we took with us a set of magnetic instruments of the Kew pattern, with a view of making observations in a district for which the magnetic elements have not hitherto been determined. The instruments employed were magnetometer No. 61, by Elliott Brothers, dip circle No. 94, by Dover, and chronometer Dent 1932. They were part of the equipment made use of in connexion with the Magnetic Survey of the British Isles.

Observations were made at Fundium, Senegal, and at Bathurst, on the River Gambia.

The results are as follows:—

Fundium, Senegal, lat. 14° 7′·4 N., long. 16° 32′ W. (approx.).

The observations were made in the vicinity of the Eclipse Camp, and on a partially enclosed piece of ground between the Administrator's house and the River Salûm, about 80 yards from the shore. The temperature during the force observations was about 30° C.

Date.	Declination.		Horizontal force.		Dip.	
	L.M.T.	Obs. result.	L.M.T.	Obs. result.	L.M.T.	Obs. result.
1893.				c.g.s.		
April 4	9.1 A.M.	18° 45′ W.	5.13 P.M.	0·30400		
" 5	8.31 "	18° 42′ "	9.26 A.M.	0·30434		
" 14	8.39 "	18° 45′ "	8.53 "	0·30394		
" 14	..	..	..	..	9.6 A.M.	Needle 1, 29° 9′·1
" 14	..	..	..	..	9.28 "	" 2, 29° 8′·2
Means ..	..	18° 44′ "	..	0·30409	..	29° 8′·7

Bathurst, River Gambia, lat. 13° 28′ N., long. 16° 37′ W.

The station was on a large piece of open ground and near the centre of McCarthy Square. All the observations taken were made on April 20, 1893.

Declination....	at 8.16 A.M. L.M.T.	=	18° 50′ W.
Horizontal force	at 8.44 "	=	0·30514 c.g.s.
Dip	at 8.17 "	=	Needle 1, 28° 43′·4.
"	at 9.14 "	=	" 2, 28° 42′·1.

III. "A certain Class of Generating Functions in the Theory of Numbers." By Major P. A. MacMahon, R.A., F.R.S. Received November 3, 1893.

(Abstract.)

The present investigation arose from my "Memoir on the Compositions of Numbers," recently read before the Royal Society and now in course of publication in the 'Philosophical Transactions.' The main theorem may be stated as follows:—

If $X_1, X_2, \ldots, X_n$ be linear functions of quantities $x_1, x_2, \ldots, x_n$ given by the matricular relation

$$(X_1, X_2, \ldots, X_n) = \left(\begin{array}{cccc} a_{11} & a_{12} & \cdot\cdot & a_{1n} \\ a_{n} & a_{22} & \cdot\cdot & a_{2n} \\ \vdots & \vdots & \ddots & \vdots \\ a_{n1} & a_{n2} & n3 & a_{nn} \end{array} \right) (x_1, x_2, \ldots, x_n),$$

that portion of the algebraic fraction

$$\frac{1}{(1-s_1X_1)(1-s_2X_2)\ldots(1-s_nX_n)},$$

which is a function of the products,

$$s_1x_1,\ s_2x_2,\ \ldots\ldots,\ s_nx_n,$$

only, is $1/V_n$, where (putting $s_1 = s_2 = \ldots\ldots = s_n = 1$)

$$V_n = (-)^n x_1x_2\ldots\ldots x_n \begin{vmatrix} a_{11}-1/x_1, & a_{12}, & \cdot\cdot & a_{1n} \\ a_{21}, & a_{22}-1/x_2 & \cdot\cdot & a_{2n} \\ \vdots & \vdots & \ddots & \vdots \\ a_{n1} & a_{n2} & \cdot\cdot & a_{nn}-1/x_n \end{vmatrix}$$

The proof of this theorem rests upon an identity which, for order 3, is

$$\begin{vmatrix} a_{11}s_1x_1-1, & a_{12}s_1x_1, & a_{13}s_1x_1, \\ a_{21}s_2x_2, & a_{22}s_2x_2-1, & a_{23}s_2x_2, \\ a_{31}s_3x_3, & a_{32}s_3x_3, & a_{33}s_3x_3-1, \end{vmatrix}$$

$$= \begin{vmatrix} 1-s_1X_1, & 0, & 0, \\ 0, & 1-s_2X_2, & 0, \\ 0, & 0, & 1-s_3X_3, \end{vmatrix}$$

$$\times \begin{vmatrix} \dfrac{s_1(a_{11}x_1-X_1)}{1-s_1X_1}-1, & \dfrac{a_{12}s_1x_1}{1-s_1X_1}, & \dfrac{a_{13}s_1x_1}{1-s_1X_1}, \\ \dfrac{a_{21}s_2x_2}{1-s_2X_2}, & \dfrac{s_2(a_{22}x_2-X)}{1-s_2X_2}-1, & \dfrac{a_{23}s_2x_2}{1-s_2X_2}, \\ \dfrac{a_{31}s_3x_3}{1-s_3X_3}, & \dfrac{a_{32}s_3x_3}{1-s_3X_3}, & \dfrac{s_3(a_{33}x_3-X_3)}{1-s_3X_3}-1, \end{vmatrix}$$

and is very easily established.

An instantaneous deduction of the general theorem is the result that the generating function for the coefficients of $x_1^{\xi_1}x_2^{\xi_2}\ldots\ldots x_n^{\xi_n}$ in the product

$$X_1^{\xi_1}X_2^{\xi_2}\ldots\ldots X_n^{\xi_n}$$

is

$$1/V_n.$$

The expression V_n involves the several coaxial minors of the determinant of the linear functions. Thus

$$V_3 = 1-a_{11}x_1-a_{22}x_2-a_{33}x_3+|a_{11}a_{22}|x_1x_2+|a_{11}a_{33}|x_1x_3+|a_{22}a_{33}|x_2x_3$$
$$-|a_{11}a_{22}a_{33}|x_1x_2x_3.$$

The theorem is of considerable arithmetical importance and is also of interest in the algebraical theories of determinants and matrices.

The product

$$X_1^{\xi_1} X_2^{\xi_2} \dots X_n^{\xi_n}$$

often appears in arithmetic as a redundant form of generating function. The theorem above supplies a condensed or exact form of generating function.

Ex. gr. It is clear that the number of permutations of the $\Sigma\xi$ symbols in the product

$$x_1^{\xi_1} x_2^{\xi_2} \dots x_n^{\xi_n}$$

which are such that every symbol is displaced is obviously the coefficient of

$$x_1^{\xi_1} x_2^{\xi_2} \dots x_n^{\xi_n}$$

in the product

$$(x_2 + \dots + x_n)^{\xi_1} (x_1 + x_3 + \dots + x_n)^{\xi_2} \dots (x_1 + x_2 + \dots + x_{n-1})^{\xi_n},$$

and thence we easily pass to the true generating function

$$\frac{1}{1 - \Sigma x_1 x_2 - 2\,\Sigma x_1 x_2 x_3 - 3\,\Sigma x_1 x_2 x_3 x_4 - \dots - (n-1)\, x_1 x_2 \dots x_n}.$$

In the paper many examples are given.

Frequently the redundant and condensed generating functions are differently interpretable; we then obtain an arithmetical correspondence, two cases of which presented themselves in the "Memoir on the Compositions of Numbers."

A more important method of obtaining arithmetical correspondences is developed in the researches which follow the statement and proof of the theorem.

The general form of V_n is such that the equation

$$V_n = 0$$

gives each quantity x_s as a homographic function of the remaining $n-1$ quantities, and it is interesting to enquire whether, assuming the coefficients of V_n arbitrarily, it is possible to pass to a corresponding redundant generating function.

I find that the coefficients of V_n must satisfy

$$2^n - n^2 + n - 2$$

conditions, and, assuming the satisfaction of these conditions, a redundant form can be constructed which involves

$$n-1$$

undetermined quantities. In fact, when a redundant form exists at all, it is necessarily of a $(n-1)$tuply infinite character.

We are now able to pass from any particular redundant generating function to an equivalent generating function which involves $n-1$ undetermined quantities. Assuming these quantities at pleasure, we obtain a number of different algebraic products, each of which may have its own meaning in arithmetic, and thus the number of arithmetical correspondences obtainable is subject to no finite limit.

This portion of the theory is given at length in the paper, with illustrative examples.

Incidentally interesting results are obtained in the fields of special and general determinant theory. The special determinant, which presents itself for examination, provisionally termed "inversely symmetric," is such that the constituents symmetrically placed in respect to the principal axis have, each pair, a product unity, whilst the constituents on the principal axis itself are all of them equal to unity. The determinant possesses many elegant properties which are of importance to the principal investigation of the paper. The theorems concerning the general determinant are connected entirely with the co-axial minors.

I find that the general determinant of even order, greater than two, is expressible in precisely two ways as an irrational function of its co-axial minors, whilst no determinant of uneven order is so expressible at all.

Of order superior to 3, it is not possible to assume arbitrary values for the determinant itself and all of its co-axial minors. In fact of order n the values assumed must satisfy

$$2^n - n^2 + n - 2$$

conditions, but, these conditions being satisfied, the determinant can be constructed so as to involve $n-1$ undetermined quantities.

IV. "On the Whirling and Vibration of Shafts." By STANLEY DUNKERLEY, M.Sc., Berkeley Fellow of the Owens College, Manchester. Communicated by OSBORNE REYNOLDS, F.R.S. Received July 13, 1893.

(Abstract.)

It is well known that every shaft, however nearly balanced, when driven at a particular speed bends, and, unless the amount of deflection be limited, might even break, although at higher speeds the shaft again runs true. The particular or "critical" speed depends on the manner in which the shaft is supported, its size and modulus of elasticity, and the size, weights and positions, of any pulleys it carries.

The theory for the case of an unloaded shaft first received attention at the hands of Professor Rankine,* who obtained numerical formulæ for the cases of a shaft resting freely on a bearing at each end, and for an overhanging shaft fixed in direction at one end.

The theory has been further extended to the case of a shaft loaded with pulleys by Professor Reynolds; and the object of this investigation is to apply that theory and so obtain formulæ—and by experiment to verify them—giving the critical speed in terms of the diameter of the shaft, weights of pulleys, &c., in particular cases applicable to the different conditions under which a shaft works.

In many cases, as might naturally be expected, the "period of whirl" of the shaft is merely its natural period of vibration. The two periods are coincident in the case of an unloaded shaft (however supported), and for a loaded shaft on which the pulleys are placed in such positions that they rotate, when the shaft is whirling, in planes perpendicular to the original alignment of the shaft. With pulleys placed in any other positions, when the shaft is whirling, there is a righting moment tending to straighten the shaft which does not exist when it merely vibrates under the dead weight of the pulleys. Hence, in an unloaded shaft, the period of whirl coincides with the natural period of vibration; but, generally, in a loaded shaft, the period of whirl is less than the natural period of vibration to an extent depending on the size and positions of the pulleys.

If, therefore, the period of disturbance (that is, the period of one revolution) be decreased, the shaft runs true until that period approximates to the natural period of vibration of the shaft under the given conditions. If the shaft now receive any displacement, however slight, a violent agitation is set up, which will be most marked when the period of disturbance and the whirling period coincide. As the period of disturbance is further decreased, the agitation becomes less and, at a period of disturbance slightly less than the whirling period of the shaft, the shaft again runs true.

As in the vibration of rods, so in the whirling of shafts, there are a series of periods at which the shaft whirls.

Investigation shows that the formulæ obtained by considering the combined effects of the shaft and only one pulley, or the combined effects of two pulleys neglecting the effect of the shaft, are too complicated—even in the simplest cases—for actual use. The only alternative method is to consider the effects of the shaft and each of the pulleys (whatever be their number, position, and size) separately, and so obtain the whirling speed for each on the assumption that all the others are neglected. By means of an empirical formula, the whirling speed taking shaft and pulleys into account may be calculated from the separately calculated speeds of whirl.

* Rankine's 'Machinery and Millwork,' p. 549.

Unloaded Shaft.—In the case of an unloaded shaft, the whirling speed is given by an equation of the form

$$(w\omega^2/g\mathrm{EI})^{\frac{1}{4}}l = a \ldots\ldots\ldots\ldots\ldots\ldots \quad (1),$$

in which

w = weight of shaft in lbs. per foot run,
g = gravity,
E = Young's Modulus (in lbs. per sq. ft.),
I = geometrical moment of inertia of cross section of shaft about a diameter,
ω = angular velocity of shaft when whirling,
l = length in feet of one of the spans, and
a = some numerical coefficient depending on the manner in which the shaft is supported.

By substituting the proper value for the coefficients in Equation (1), we obtain the equations

$$\mathrm{N} = b\, d/l^2, \text{ for a solid shaft} \ldots\ldots\ldots\ldots\ldots\ldots \quad (2),$$

and

$$= b\sqrt{(d_1^2 + d_2^2)}/l^2, \text{ for a hollow shaft} \ldots\ldots \quad (3).$$

In these equations

N = number of revolutions shaft makes, per minute, when whirling,
d = diameter of the solid shaft in inches,
d_1, d_2 = the external and internal diameters of the hollow shaft, both in inches, and
b = some numerical coefficient depending on a, and also upon

the constants in Equation (1).

For wrought iron or mild steel, $b = 3331\,a^2$ and for brass, $2222\,a^2$.

It should be noticed that each span of a continuous shaft supported on bearings placed at equal distances apart whirls independently of the rest.

Loaded Shafts.—The cases for a loaded shaft—as also those for an unloaded one—which have been considered in the investigation, embrace all those which are likely to arise in practice. With a continuous shaft supported on three or more bearings and loaded with a pulley on one of the spans, general equations have been obtained for unequal spans, but the complete investigation has only been extended to the case of equal spans—a relation between the spans which almost invariably exists in actual shafting. The results of the investigation show that however the shaft be supported, the whirling speed due to a single pulley on a light shaft is given by an equation of the form

$$\omega = \theta\sqrt{(g\mathrm{EI}/\mathrm{W}c^3)} \ldots\ldots\ldots\ldots\ldots\ldots \quad (4),$$

in which ω, g, E, I have the same meanings as in Equation (1), and

W = weight of pulley in lbs.;
c = distance of pulley from nearest bearing in feet; and
θ = some numerical coefficient depending not only upon the manner in which the shaft is supported, but also upon the position of the pulley on the span which carries it and the size of the pulley.

For any particular mode of support of the shaft the coefficient θ is some function of the ratios c/l and c/k, where l = length of span which carries the pulley, and $k = \sqrt{g\,(A-B)/W}$, where A and B are the moments of inertia of the pulley about the axis of the shaft and a diameter of the pulley through its centre of gravity respectively, both being expressed in gravitation units. Assuming, therefore, certain values for c/l and c/k, tables have been drawn up giving the corresponding values of θ. For each value of c/l there are two limiting values of c/k, viz., *infinity* and *zero*—the corresponding values of k being *zero* and *infinity*. When $k = 0$ the pulley may be considered simply as a dead weight, so that the "inferior period of whirl," as it has been termed, is the natural period of vibration of the light shaft under the given conditions. The "superior period of whirl," that is to say, the whirling period when $k = \infty^y$, is the inferior period multiplied by some function of c/l, and assuming the shaft to whirl at a speed corresponding to the superior or inferior limit, it would do so in such a manner that the pulley still rotated in a plane perpendicular to the original alignment of the shaft. These limiting values of the speed, below and above which whirling is impossible, have been calculated in each case.

Investigation shows that in a continuous light shaft supported on bearings placed at equal distances apart, the increase in the whirling speed due to those spans which are not immediately adjacent to the loaded one, on either side, cannot exceed by above 2 or 3 per cent. the whirling speed when only the loaded span and the spans immediately adjacent to it are taken into account. In other words, the stiffening effects of only those spans immediately adjacent to the loaded one, on either side, need be taken into account in calculating the whirling speed for the shaft.

By substituting the proper values for the constants in Equation (4) we obtain the equations

$$N = \phi\, d^2/\sqrt{Wc^3}, \text{ for a solid shaft} \ldots\ldots\ldots\ldots \quad (5),$$

and

$$= \phi\, \sqrt{(d_1^4 - d_2^4)}/\sqrt{Wc^3}, \text{ for a hollow shaft} \ldots\ldots \quad (6),$$

where W, d, d_1, d_2 have the same values as before, and ϕ is some numerical coefficient depending on the value of θ and of the constants in Equation (4).

The value of ϕ for wrought-iron or mild steel is 5389θ, and for brass 3772θ.

Having thus obtained formulæ giving the whirling speed due to each cause, on the assumption that all the others are neglected, it only remains to find an empirical formula giving the resulting whirling speed, when all the disturbing elements are taken into account, in terms of the separately calculated whirling speeds due to the several causes. Since the whirling speed in every case varies inversely as the square root of the weight of the pulley (*see* Equation 4), the formula for calculating the resulting speed was taken to be of the form

$$N_1N_2/\sqrt{(N_1^2+N_2^2)} \quad \ldots\ldots\ldots\ldots\ldots\ldots \quad (7)$$

for two disturbing elements whose whirling speeds taken separately are N_1, N_2; or of

$$N_1N_2N_3/\sqrt{(N_2^2N_3^2+N_3^2N_1^2+N_1^2N_2^2)} \quad \ldots\ldots\ldots\ldots \quad (8)$$

for three disturbing elements whose whirling speeds, taken separately, are N_1, N_2, N_3. The formula may be extended to any number of disturbing elements. It is not strictly accurate, for in addition to the whirling speed varying inversely as the square root of the weight of the pulley, it also varies as some function (θ) of the distance of the pulley from the nearest bearing and of the size of the pulley. The experiments, however, justify to a remarkable degree the assumptions that have been made in calculating the resulting whirling speed.

In calculating the speed at which a continuous shaft of given diameter, supported on bearings placed at equal distances apart, and loaded with pulleys on any or all of the spans, will whirl, the method to adopt is to, first, find the span which will have the biggest whirl (that is to say the span which carries the heaviest and most advantageously-placed pulleys as regards whirling), and then to consider this span and the spans immediately adjacent on either side. The whirling speeds for the shaft and each of the pulleys on the three spans have then to be calculated according to the rules laid down in each case. The whirling speed for any pulleys on the two side spans will, of course, be different according as that side span is an end or an intermediate span in the line of shafting. Having found the whirling speed due to each cause, the resulting whirling speed is found from an equation of the same form as Equation (7) or (8)—the exact equation depending on the number of disturbing elements. The speed thus obtained will be slightly less than the actual whirling speed. A nearer approximation to the actual speed might be obtained by considering only those pulleys which lie near the centres, or between the centres of the side spans and the bearings of the middle span, neglecting the effect of those pulleys which lie beyond

the centres of the side spans. In doing so, however, the experiments show that there is a danger of the calculated speed *exceeding* the actual, whilst by taking *all* the pulleys on the two side spans into account the calculated speed will be slightly *less* than the actual speed.

If the spans of a continuous shaft, supported on bearings placed at equal distances apart, are all loaded in the same manner, each whirls independently of the rest, and the problem reduces to that of a loaded shaft supported on bearings at the ends.

The *experimental apparatus* by which the calculated results have been, for the most part, verified is shown in a figure. The experimental shaft was 2 ft. 8 in. long and 0·2488 in. diameter. The motion was transmitted from the headstock spindle to the experimental shaft by a fine piece of steel wire (about $1\frac{1}{2}$ in. long and 21 B.W.G. diam.), so that the shaft was subjected to very little constraint at the end. The experimental pulleys were models of actual pulleys—being designed for both weight and inertia. The headstock spindle was driven from a turbine, the constancy of the speed being shown by the steadiness of a column of liquid forced by a centrifugal fan indicator up a glass tube. In taking the number of revolutions corresponding to any period of whirl an ordinary counter pushed into the end of the headstock spindle was used. In making any experiment three trials were made (each of three minutes' duration) and the mean of the results taken. Over 150 experiments have been made with this apparatus, and the observed results invariably approximate very closely to the calculated results. Experiments have also been made with actual cases of shafting, and it would appear that, following the method of solution sketched above, the calculated speed is about 3 or 4 per cent. *less* than the actual speed.

The experiments were carried out in the Whitworth Engineering Laboratory, the Owens College, Manchester.

V. "On Plane Cubics." By CHARLOTTE ANGAS SCOTT, D.Sc. (Lond.), Professor of Mathematics at Bryn Mawr College, Pennsylvania. Communicated by A. R. FORSYTH, Sc.D., F.R.S. Received September 9, 1893.

(Abstract.)

In this paper the first few sections are devoted to certain constructions for the cubic, its Hessian, and its Cayleyan. Assuming three collinear inflexions for the cubic, and the tangents at these points, *i.e.*, eight conditions, one more point determines the cubic, and, consequently, also the Hessian and Cayleyan. Taking

this point on one of the known harmonic polars, the remaining two points in which the harmonic polar meets the cubic are found by a quadratic construction, and triangular symmetry completes the determination of the cubic; the inflexional tangents to the Hessian and the cusps on the Cayleyan are found by linear constructions; and the pairs of points in which these curves are met by the harmonic polar, by quadratic constructions. Any coincidence among the points so found indicates some special cubic whose properties may thus be investigated; among these special cubics are the equianharmonic cubics, whose properties present themselves very simply by means of the preliminary constructions. These special cubics are the critical ones when we follow out the variation in the Hessian and Cayleyan, which, depending directly on the variation in the original cubic, expresses itself by the change in the relative position of the points determined as above. This variation is shown in a series of diagrams exhibiting the cubic, its Hessian, and Cayleyan; and, finally, the results being compared with those derived from analysis, the variation is represented graphically by means of a single diagram.

VI. "Alternate Current Electrolysis." By J. HOPKINSON, D.Sc., F.R.S., E. WILSON, and F. LYDALL. Received November 2, 1893.

[Publication deferred.]

Presents, November 23, 1893.

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November 30, 1893.

ANNIVERSARY MEETING.

The LORD KELVIN, D.C.L., LL.D., President, in the Chair.

The Report of the Auditors of the Treasurer's Accounts, on the part of the Society, was presented as follows:—

"The total receipts on the General Account during the past year, including balances carried from the preceding year and the proceeds of the sale of stock, amount to £24,184, and the total receipts on account of Trust Funds, including balances from the preceding year, amount to £7,255 0*s*. 11*d*. The total expenditure for the same period, including investments, amounts to £21,298 8*s*. 8*d*. on the General Account, and £4,585 1*s*. 6*d*. on account of Trust Funds, leaving a balance on the General Account of £2,875 16*s*. 6*d*. at the bankers', and £10 3*s*. 4*d*. in the hands of the Treasurer, and a balance at the bankers' on account of Trust Funds of £2,669 19*s*. 5*d*."

The thanks of the Society were voted to the Treasurer and Auditors.

The Secretary then read the following Lists:—

Fellows deceased since the last Anniversary (Nov. 30, 1892).

On the Home List.

Blanford, Henry Francis, F.G.S.
Clark, Sir Andrew, Bart., M.D.
Derby, Henry Edward Stanley, Earl of, K.G.
Fletcher, Thomas William, Colonel, M.A.
Hawksley, Thomas, M. Inst. C.E.
Jago, James, M.D.
Owen, Sir Richard, K.C.B.
Pritchard, Rev. Charles, D.D.
Rae, John, LL.D.
Stainton, Henry Tibbats, F.L.S.
Walker, Edward, M.A.

On the Foreign List.

De Candolle, Alphonse.
Kummer, Ernst Eduard.

Change of Name and Title.

Bowen, Sir Charles, *to* Lord Bowen.

Fellows elected since the last Anniversary.

Royal.

H.R.H. the Duke of York, K.G.

Burnside, Professor William, M.A.
Dunstan, Professor Wyndham R., M.A.
Ellis, William, F.R.A.S.
Ewart, Professor J. Cossar, M.D.
Gairdner, Professor William Tennant, M.D.
Hobson, Ernest William, D.Sc.
Howorth, Sir Henry Hoyle, K.C.I.E.
Morley, Right Hon. John, M.A.
Newton, Edwin Tulley, F.G.S.
Sherrington, Charles Scott, M.D.
Stirling, Edward C., M.D.
Thornycroft, John Isaac, M. Inst. C.E.
Trail, Professor James William Helenus, M.D.
Wallace, Alfred Russel, LL.D.
Worthington, Professor Arthur Mason, M.A.
Young, Professor Sydney, D.Sc.

The President then addressed the Society as follows:—

Since our last Anniversary Meeting, the Royal Society has lost 11 Fellows on the Home List, and 2 Foreign Members.

Henry Tibbats Stainton, December 2, 1892, aged 70.
Sir Richard Owen, December 18, 1892, aged 89.
Dr. James Jago, January 18, 1893, aged 77.
Henry Francis Blanford, January 23, 1893, aged 58.
Thomas William Fletcher, February 1, 1893, aged 84.
Edward Walker, March 2, 1893, aged 73.
Alphonse de Candolle, March 28, 1893, aged 87.
Henry Edward Stanley, Earl of Derby, April 21, 1893, aged 67.
Ernest Edward Kummer, May 14, 1893, aged 84.
Rev. Charles Pritchard, May 28, 1893, aged 85.
Dr. John Rae, July 22, 1893, aged 80.
Thomas Hawksley, September 23, 1893, aged 86.
Sir Andrew Clark, Bart., November 6, 1893, aged 67.

Biographical notices will be found in the Proceedings.

During the past session our standing committees have been as active as ever. The Library Committee have again had before them the question of finding accommodation to meet the rapid growth of our Library. One measure which, with the consent of the Council, they have adopted to this end has been to part with a number of the literary and philosophical series of transactions published by those societies which are not, like our own, purely scientific. In some cases these series are being returned to the institutions who gave them; in others, where this is not desired, they are presented to libraries in which they will be of greater use than in our own.

The House and Soirée Committee have held more than their usual number of meetings; and, acting upon their advice, the Council ap-

pointed a Special Committee to arrange for the better accommodation of the Fellows at their ordinary meetings and of their visitors at the Annual Soirées. Upon the first floor a new doorway has been provided, which, it is hoped, will help to a freer circulation on the crowded nights of our Soirées, and on the ground floor, besides the rearrangement of the meeting-room and the provision of a lecture table, and additional accommodation for diagrams, a preparation room is being fitted with suitable appliances for the use of those who are willing to illustrate their papers by experiment.

The generous gift of £2000 presented by our Fellow Mr. Ludwig Mond, in the early part of the session, to aid the work of the 'Catalogue of Scientific Papers,' has enabled the Catalogue Committee not only to carry on the current work of the Catalogue, which want of funds threatened to cripple, but also to take into consideration wider schemes than it was possible to contemplate before. The Committee have met several times during the past session, and it is hoped that the long-desired Subject Index may yet become an accomplished fact.

The Water Research Committee have continued their labours, and a Second Report on the vitality of microscopic pathogenic organisms in large bodies of water, dealing with the vitality and virulence of *Bacillus anthracis* and its spores, the result of Messrs. Percy Frankland and Marshall Ward's researches, has been completed during the past session and published in our 'Proceedings.'

Except that additional assistants have been employed in the Catalogue Department, our staff remains unchanged.

During the past year, in the Mathematical and Physical Section of the 'Philosophical Transactions,' 21 papers have been published, and in the Biological Section, 10; the two sections together containing a total of 1775 pages of letter-press, and 70 plates. Of the 'Proceedings,' 12 numbers have been issued, containing 1282 pages and 19 plates.

Not the least important of the scientific events of the year is the publication, in the original German and in an English translation by Professor D. E. Jones, of a collection of Hertz's papers describing the researches by which he was led up to the experimental demonstration of magnetic waves. For this work the Rumford Medal of the Royal Society was delivered to Professor Hertz three years ago by my predecessor, Sir George Stokes. To fully appreciate the book now given to the world, we must carry our minds back to the early days of the Royal Society, when Newton's ideas regarding the forces which he saw to be implied in Kepler's laws of the motions of the Planets and of the Moon were frequent subjects of discussion at its regular meetings and at perhaps even more important non-official conferences among its fellows.

In 1684 the Senior Secretary of the Royal Society, Dr. Halley,

went to Cambridge to consult Mr. Newton on the subject of the production of the elliptic motion of the Planets by a central force,* and on the 10th of December of that year he announced to the Royal Society that he "had seen Mr. Newton's book, 'De Motu Corporum.'" Some time later, Halley was requested to "remind Mr. Newton of his promise to enter an account of his discoveries in the register of the Society," with the result that the great work 'Philosophiæ Naturalis Principia Mathematica' was dedicated to the Royal Society, was actually presented in manuscript, and was communicated at an ordinary meeting of the Society on the 28th of April 1686 by Dr. Vincent. In acknowledgment, it was ordered "that a letter of thanks be written to Mr. Newton, and that the printing of his book be referred to the consideration of the Council; and that in the meantime the book be put into the hands of Mr. Halley, to make a report thereof to the Council." On the 19th of May following, the Society resolved that "Mr. Newton's 'Philosophiæ Naturalis Principia Mathematica' be printed forthwith in quarto, in a fair letter; and that a letter be written to him to signify the Society's resolution, and to desire his opinion as to the volume, cuts, &c." An exceedingly interesting letter was accordingly written to Newton by Halley, dated London, May 22, 1686, which we find printed in full in Weld's 'History of the Royal Society' (vol. 1, pp. 308—309). But the Council knew more than the Royal Society at large of its power to do what it wished to do. Biology was much to the front then, as now, and the publication of Willughby's book, 'De Historia Piscium,' had exhausted the Society's finances to such an extent that the salaries even of its officers were in arrears. Accordingly, at the Council meeting of the 2nd of June, it was ordered that "Mr. Newton's book be printed, and that Mr. Halley undertake the business of looking after it, and printing it at his own charge, which he engaged to do."

It seems that at that time the office of Treasurer must have been in abeyance; but with such a Senior Secretary as Dr. Halley there was no need for a Treasurer.

Halley, having accepted copies of Willughby's book, which had been offered to him in lieu of payment of arrears of salary†

* Whewell's 'History of the Inductive Sciences,' vol. 2, p. 77.

† It is recorded in the Minutes of Council that the arrears of salary due to Hooke and Halley were resolved to be paid by copies of Willughby's work. Halley appears to have assented to this unusual proposition, but Hooke wisely "desired six months' time to consider of the acceptance of such payment."

The publication of the 'Historia Piscium,' in an edition of 500 copies, cost the Society £400. It is worthy of remark, as illustrative of the small sale which scientific books met with in England at this period, that, a considerable time after the publication of Willughby's work, Halley was ordered by the Council to endeavour to effect a sale of several copies with a bookseller at Amsterdam, as appears in a letter

due to him, cheerfully undertook the printing of the 'Principia' at his own expense, and entered instantly on the duty of editing it with admirable zeal and energy, involving, as it did, expostulations, arguments, and entreaties to Newton not to cut out large parts of the work which he wished to suppress* as being too slight and popular, and as being possibly liable to provoke questions of priority. It was well said by Rigaud, in his 'Essay on the first publication of the Principia,' that "under the circumstances, it is hardly possible to form a sufficient estimate of the immense obligation which the world owes in this respect to Halley, without whose great zeal, able management, unwearied perseverance, scientific attainments, and disinterested generosity, the 'Principia' might never have been published."† Those who know how much worse than "law's delays" are the troubles, cares, and labour involved in bringing through the press a book on any scientific subject at the present day will admire Halley's success in getting the 'Principia' published within about a year after the task was committed to him by the Royal Society, two hundred years ago.

When Newton's theory of universal gravitation was thus made known to the world Descartes' *Vortices*, an invention supposed to be a considerable improvement on the older invention of crystal cycles and epi-cycles from which it was evolved, was generally accepted, and seems to have been regarded as quite satisfactory by nearly all the philosophers of the day.

The idea that the Sun pulls Jupiter, and Jupiter pulls back against the Sun with equal force, and that the Sun, Earth, Moon, and Planets all act on one another with mutual attractions, seemed to violate the supposed philosophic principle that matter cannot act where it is not. Descartes's doctrine died hard among the mathematicians and philosophers of Continental Europe; and for the first quarter of last century belief in universal gravitation was an insularity of our countrymen.

from Halley requesting Boyle, then at Rotterdam, to do all in his power to give publicity to the book. When the Society resolved on Halley's undertaking to measure a degree of the Earth, it was voted that "he be given £50 or fifty 'Books of Fishes'" (Weld's 'History of the Royal Society,' vol. 1, p. 310).

* "The third [book] I now design to suppress. Philosophy is such an impertinently litigious lady that a man had as good be engaged in lawsuits as have to do with her. I found it so formerly, and now I am no sooner come near her again but she gives me warning. The first two books without the third will not so well bear the title of 'Philosophiæ Naturalis Principia Mathematica,' and therefore I have altered it to this, 'De Motu Corporum Libri duo"; but, upon second thoughts, I retain the former title. 'Twill help the sale of the book, which I ought not to diminish now 'tis yours" (*ibid.*, p. 311).

† *Ibid.*, p. 310.

Voltaire, during a visit which he made to England in 1727, wrote: "A Frenchman who arrives in London finds a great alteration in philosophy, as in other things. He left the world full; he finds it empty. At Paris you see the universe composed of vortices of subtle matter; at London we see nothing of the kind. With you it is the pressure of the Moon which causes the tides of the sea; in England it is the sea which gravitates towards the Moon. . . . You will observe also that the Sun, which in France has nothing to do with the business, here comes in for a quarter of it. Among you Cartesians all is done by impulsion: with the Newtonians it is done by an attraction of which we know the cause no better."* Indeed, the Newtonian opinions had scarcely any disciples in France till Voltaire asserted their claims on his return from England in 1728. Till then, as he himself says, there were not twenty Newtonians out of England.†

In the second quarter of the century sentiment and opinion in France, Germany, Switzerland, and Italy experienced a great change. The mathematical prize questions proposed by the French Academy naturally brought the two sets of opinions into conflict. A Cartesian memoir of John Bernoulli was the one which gained the prize in 1730. It not infrequently happened that the Academy, as if desirous to show its impartiality, divided the prize between Cartesians and Newtonians. Thus, in 1734, the question being the cause of the inclination of the orbits of the planets, the prize was shared between John Bernoulli, whose memoir was founded on the system of vortices, and his son Daniel, who was a Newtonian. The last act of homage of this kind to the Cartesian system was performed in 1740, when the prize on the question of the tides was distributed between Daniel Bernoulli, Euler, Maclaurin, and Cavallieri; the last of whom had tried to amend and patch up the Cartesian hypothesis on this subject.‡

On the 4th of February, 1744, Daniel Bernoulli wrote as follows to Euler: "Uebrigens glaube ich, dass der Aether sowohl *gravis versus solem*, als die Luft versus terram sey, und kann Ihnen night bergen, dass ich über diese Puncte ein völliger Newtonianer bin, vnd verwundere ich mich, dass sie den Principiis Cartesianis so lang adhäriren; es möchte wohl einige Passion vielleicht mit unterlaufen. Hat Gott können eine *animam*, deren Natur uns unbegreiflich ist, erschaffen, so hat er auch können eine attractionem universalem materiae imprimiren, wenn gleich solche attractio *supra captum* ist, da hingegen die Principia Cartesiana allzeit *contra captum* etwas involviren."

Here the writer, expressing wonder that Euler had so long adhered

* Whewell's 'History of the Inductive Sciences,' vol. 2, pp. 202—203.

† *Ibid.*, vol. 2, p. 201.

‡ *Ibid.*, vol. 2, pp. 198, 199.

to the Cartesian principles, declares himself a thorough-going Newtonian, not merely in respect to gravitation *versus* vortices, but in believing that matter may have been created simply with the law of universal attraction without the aid of any gravific medium or mechanism. But in this he was more Newtonian than Newton himself.

Indeed Newton was not a Newtonian, according to Daniel Bernoulli's idea of Newtonianism, for in his letter to Bentley of date 25th February, 1692,* he wrote: "That gravity should be innate, inherent, and essential to matter, so that one body may act upon another at a distance through a vacuum without the mediation of anything else, by and through which their action and force may be conveyed from one to another, is to me so great an absurdity that I believe no man who has in philosophical matters a competent faculty of thinking can ever fall into it." Thus Newton, in giving out his great law, did not abandon the idea that matter cannot act where it is not. In respect, however, merely of philosophic thought, we must feel that Daniel Bernoulli was right; we can conceive the Sun attracting Jupiter, and Jupiter attracting the Sun, without any intermediate medium, if they are ordered to do so. But the question remains—Are they so ordered? Nevertheless, I believe all, or nearly all, his scientific contemporaries agreed with Daniel Bernoulli in answering this question affirmatively. Very soon after the middle of the eighteenth century Father Boscovich† gave his brilliant doctrine (if infinitely improbable theory) that elastic rigidity of solids, the elasticity of compressible liquids and gases, the attractions of chemical affinity and cohesion, the forces of electricity and magnetism—in short, all the properties of matter except heat, which he attributed to a sulphureous fermenting essence—are to be explained by mutual attractions and repulsions, varying solely with distances, between mathematical points endowed also, each of them, with inertia. Before the end of the eighteenth century the idea of action-at-a-distance through absolute vacuum had become so firmly established, and Boscovich's theory so unqualifiedly accepted as a reality, that the idea of gravitational force or electric force or magnetic force being propagated through and by a medium seemed as wild to the naturalists and mathematicians of 100 years ago as action-at-a-distance had seemed to Newton and his contemporaries 100 years earlier. But a retrogression from the eighteenth century school of science set in early in the nineteenth century.

Faraday, with his curved lines of electric force, and his dielectric

* 'The Correspondence of Richard Bentley, D.D.,' vol. 1, p. 70.

† Theoria Philosophiæ Naturalis redacta ad unicam legem virium in natura existentium auctore P. Rogerio Josepho Boscovich, Societatis Jesu,' 1st edition, Vienna, 1758; 2nd edition, amended and extended by the author, Venice, 1763.

efficiency of air and of liquid and solid insulators, resuscitated the idea of a medium through which, and not only through which but *by* which, forces of attraction or repulsion, seemingly acting at a distance, are transmitted. The long struggle of the first half of the eighteenth century was not merely on the question of a medium to serve for gravific mechanism, but on the correctness of the Newtonian law of gravitation as a matter of fact however explained. The corresponding controversy in the nineteenth century was very short, and it soon became obvious that Faraday's idea of the transmission of electric force by a medium not only did not violate Coulomb's law of relation between force and distance, but that, if real, it must give a thorough explanation of that law.* Nevertheless, after Faraday's discovery† of the different specific inductive capacities of different insulators, twenty years passed before it was generally accepted in Continental Europe. But before his death, in 1867, he had succeeded in inspiring the rising generation of the scientific world with something approaching to faith that electric force is transmitted by a medium called ether, of which, as had been believed by the whole scientific world for 40 years, light and radiant heat are transverse vibrations. Faraday himself did not rest with this theory for electricity alone. The very last time I saw him at work in the Royal Institution was in an underground cellar, which he had chosen for freedom from disturbance; and he was arranging experiments to test the time of propagation of magnetic force from an electro-magnet through a distance of many yards of air to a fine steel needle polished to reflect light; but no result came from those experiments. About the same time or soon after, certainly not long before the end of his working time, he was engaged (I believe at the shot tower near Waterloo Bridge on the Surrey side) in efforts to discover relations between gravity and magnetism, which also led to no result.

Absolutely nothing has hitherto been done for gravity either by experiment or observation towards deciding between Newton and Bernoulli, as to the question of its propagation through a medium, and up to the present time we have no light, even so much as to point a way for investigation, in that direction. But for electricity and magnetism, Faraday's anticipations and Clerk-Maxwell's splendidly developed theory have been established on the sure basis of experiment by Hertz's work, of which his own most interesting account is this year presented to the world in the German and English volumes to which I have referred. It is interesting to know, as Hertz explains in his introduction, and it is very important in respect to the experi-

* 'Electrostatics and Magnetism,' Sir W. Thomson, Arts. I (1842) and II (1845), particularly § 25 of Art. II.

† 1837, 'Experimental Researches,' 1161—1306.

mental demonstration of magnetic waves to which he was led that he began his electric researches in a problem happily put before him thirteen years ago by Professor von Helmholtz, of which the object was to find by experiment some relation between electromagnetic forces and dielectric polarisation of insulators, without, in the first place, any idea of discovering a progressive propagation of those forces through space.

It was by sheer perseverance in philosophical experimenting that Hertz was led to discover a finite velocity of propagation of electromagnetic action, and then to pass on to electromagnetic waves in air and their reflection, and to be able to say, as he says in a short reviewing sentence at the end of his eighth paper: "Certainly it is a fascinating idea that the processes in air which we have been investigating, represent to us on a million-fold larger scale the same processes which go on in the neighbourhood of a Fresnel mirror, or between the glass plates used for exhibiting Newton's rings."

Professor Oliver Lodge has done well, in connection with Hertz's work, to call attention* to old experiments, and ideas taken from them, by Joseph Henry, which came more nearly to an experimental demonstration of electromagnetic waves than anything that had been done previously. Indeed Henry, after describing experiments showing powerful enough induction due to a single spark from the prime conductor of an electric machine to magnetise steel needles at a distance of 30 ft. in a cellar beneath with two floors and ceilings intervening, says that he is "disposed to adopt the hypothesis of an electrical plenum," and concludes with a short reviewing sentence: "It may be further inferred that the diffusion of motion in this case is almost comparable with that of a spark from a flint and steel in the case of light."

Professor Oliver Lodge himself did admirable work in his investigations with reference to lightning rods,† coming very near to experimental demonstrations of electromagnetic waves; and he drew important lessons regarding "electrical surgings" in an insulated bar of metal "induced by Maxwell's and Heaviside's electromagnetic waves," and many other corresponding phenomena manifested both in ingenious and excellent experiments devised by himself and in natural effects of lightning.

Of electrical surgings or waves in a short insulated wire, and of interference between ordinary and reflected waves, and positive electricity appearing where negative might have been expected, we hear first, it seems, in Herr von Bezold's "Researches on the Electric Discharge" (1870), which Hertz gives as the third paper of his

* 'Modern Views of Electricity,' pp. 369—372.

† 'Lightning Conductors and Lightning Guards,' Oliver J. Lodge, D.Sc., F.R.S. Whittaker and Co.

collection, with interesting and ample recognition of its importance in relation to his own work.

In connexion with the practical development of magnetic waves, you will, I am sure, be pleased if I call your attention to two papers by Professor G. F. Fitzgerald, which I heard myself at the meeting of the British Association at Southport, in 1883. One of them is entitled "On a Method of Producing Electromagnetic Disturbances of comparatively Short Wave-lengths." The paper itself is not long, and I shall read it to you in full, from the 'Report of the British Association,' 1883: "This is by utilising the alternating currents produced when an accumulator is discharged through a small resistance. It is possible to produce waves of as little as 2 metres wave-length, or even less." This was a brilliant and useful suggestion. Hertz, not knowing of it, used the method; and, making as little as possible of the "accumulator," got waves of as little as 24 cm. wave-length in many of his fundamental experiments. The title alone of Fitzgerald's other paper, "On the Energy Lost by Radiation from Alternating Currents," is in itself a valuable lesson in the electromagnetic theory of light, or the undulatory theory of magnetic disturbance. It is interesting to compare it with the title of Hertz's eleventh paper, "Electric Radiation"; but I cannot refer to this paper without expressing the admiration and delight with which I see the words "rectilinear propagation," "polarisation," "reflection," "refraction," appearing in it as sub-titles.

During the 56 years which have passed since Faraday first offended physical mathematicians with his curved lines of force, many workers and many thinkers have helped to build up the nineteenth century school of *plenum;* one ether for light, heat, electricity, magnetism; and the German and English volumes containing Hertz's electrical papers, given to the world in the last decade of the century, will be a permanent monument of the splendid consummation now realised.

But, splendid as this consummation is, we must not fold our hands and think or say there are no more worlds to conquer for electrical science. We do know something now of magnetic waves. We know that they exist in nature and that they are in perfect accord with Maxwell's beautiful theory. But this theory teaches us nothing of the actual motions of matter constituting a magnetic wave. Some definite motion of matter perpendicular to the lines of alternating magnetic force in the waves and to the direction of propagation of the action through space, there must be; and it seems almost satisfactory as a hypothesis to suppose that it is chiefly a motion of ether with a comparatively small but not inconsiderable loading by fringes of ponderable molecules carried with it. This makes Maxwell's "electric displacement" simply a to-and-fro motion of ether across the line

of propagation, that is to say, precisely the vibrations in the undulatory theory of light according to Fresnel. But we have as yet absolutely no guidance towards any understanding or imagining of the relation between this simple and definite alternating motion, or any other motion or displacement of the ether, and the earliest known phenomena of electricity and magnetism—the electrification of matter, and the attractions and repulsions of electrified bodies; the permanent magnetism of lodestone and steel, and the attractions and repulsions due to it: and certainly we are quite as far from the clue to explaining, by ether or otherwise, the enormously greater forces of attraction and repulsion now so well known after the modern discovery of electromagnetism.

Fifty years ago it became strongly impressed on my mind that the difference of quality between vitreous and resinous electricity, conventionally called positive and negative, essentially ignored as it is in the mathematical theories of electricity and magnetism with which I was then much occupied (and in the whole science of magnetic waves as we have it now), must be studied if we are to learn anything of the nature of electricity and its place among the properties of matter. This distinction, essential and fundamental as it is in frictional electricity, electro-chemistry, thermo-electricity, pyro-electricity of crystals, and piezo-electricity of crystals, had been long observed in the old known beautiful appearances of electric glow and brushes and sparks from points and corners on the conductors of ordinary electric machines and in exhausted receivers of air-pumps with electricity passed through them. It was also known, probably as many as fifty years ago, in the vast difference of behaviour of the positive and negative electrodes of the electric arc lamp. Faraday gave great attention to it* in experiments and observations regarding electric sparks, glows, and brushes, and particularly in his "dark discharge" and "dark space" in the neighbourhood of the negative electrode in partial vacuum. In [1523] of his 12th series, he says, "The results connected with the different conditions of positive and negative discharge will have a far greater influence on the philosophy of electrical science than we at present imagine." His "dark discharge" ([1544—1554]) through space around or in front of the negative electrode was a first instalment of modern knowledge in that splendid field of experimental research which, fifteen years later, and up to the present time, has been so fruitfully cultivated by many of the ablest scientific experimenters of all countries.

The Royal Society's Transactions and Proceedings of the last forty years contain, in the communications of Gassiot,† Plücker,‡ Andrews

* 'Experimental Researches,' Series 12 and 13, Jan. and Feb., 1838.

† 'Roy. Soc. Proc.,' vol. 10, 1860, pp. 36, 269, 274, 432.

‡ 'Roy. Soc. Proc.,' vol. 10, 1860, p. 256.

and Tait,* Robinson,† Cromwell Varley,‡ De la Rue and Müller,§ Spottiswoode,|| Moulton,¶ Grove,** Crookes,†† Schuster,‡‡ J. J. Thomson,§§ and Fleming,|||| almost a complete history of the new province of electrical science which has grown up, largely in virtue of the great modern improvements in practical methods for exhausting air from glass vessels, culminating in Sprengel's mercury-shower pump, by which we now have "vacuum tubes" and bulbs containing less than 1/190,000 of the air which would be left in them by all that could be done in the way of exhausting (supposed to be down to 1 mm. of mercury) by the best air-pump of fifty years ago. A large part of the fresh discoveries in this province has been made by the authors of these communications; and their references to the discoveries of other workers very nearly complete the history of all that has been done in the way of investigating the transmission of electricity through highly rarefied air and gases since the time of Faraday.

Varley's short paper of 1871, which, strange to say, has lain almost or quite unperceived in the Proceedings during the twenty-two years since its publication, contains an important first instalment of discovery in a new field—the molecular torrent from the "negative pole," the control of its course by a magnet, its pressure against either end of a pivoted vane of mica according as it is directed by a magnet to one end or the other, and the shadow produced by its interception by a mica screen. Quite independently of Varley, and not knowing what he had done, Crookes was led to the same primary discovery, not by accident, and not merely by experimental skill and acuteness of observation. He was led to it by carefully designed investigation,

* 'Roy. Soc. Proc.,' vol. 10, 1860, p. 274; 'Phil. Trans.,' 1860, p. 118.

† 'Roy. Soc. Proc.,' vol. 12, 1862, p. 202.

‡ 'Roy. Soc. Proc.,' vol. 19, 1871, p. 236.

§ 'Roy. Soc. Proc.,' vol. 23, 1875, p. 356; vol. 26, 1877, p. 519; vol. 27, 1878, p. 374; vol. 29, 1879, p. 281; vol. 35, 1883, p. 292; vol. 36, 1884, pp. 151, 206; 'Phil. Trans.,' 1878, pp. 55, 155; 1880, p. 65; 1883, 477.

|| 'Roy. Soc. Proc.,' vol. 23, 1875, pp. 356, 455; vol. 25, 1875, pp. 73, 547; vol. 26, 1877, pp. 90, 323; vol. 27, 1878, p. 60; vol. 29, 1879, p. 21; vol. 30, 1880 p. 302; vol. 32, 1881, pp. 385, 388; vol. 33, 1882, p. 423; 'Phil. Trans.,' 1878, pp. 163, 210; 1879, 165; 1880, p. 561.

¶ 'Roy. Soc. Proc.,' vol. 29, 1879, p. 21; vol. 30, 1880, p. 302; vol. 32, 1881, pp. 385, 388; vol. 33, 1882, p. 453; 'Phil. Trans.,' 1879, p. 165; 1880, p. 561.

** 'Roy. Soc. Proc.,' vol. 28, 1878, p. 181.

†† 'Roy. Soc. Proc.,' vol. 28, 1879, pp. 347, 477; 'Phil. Trans.,' 1879, p. 641; 1880, p. 135; 1881, 387.

‡‡ 'Roy. Soc. Proc.,' vol. 37, 1884, pp. 78, 317; vol. 42, 1887, p. 371; vol. 47, 1890; pp. 300, 506.

§§ 'Roy. Soc. Proc.,' vol. 42, 1887, p. 343; vol. 49, 1891, p. 84.

|||| 'Roy. Soc. Proc.,' vol. 47, 1890, p. 118.

starting with an examination of the cause of irregularities which had troubled* him in his weighing of thallium; and, going on to trials for improving Cavendish's gravitational measurement, in the course of which he discovered that the seeming attraction by heat is only found in air of greater than 1/1000† of ordinary density; and that there is repulsion increasing to a maximum when the density is decreased from 1/1000 to 36/1,000,000, and thence diminishing towards zero as the rarefaction is farther extended to density 1/20,000,000. From this discovery Crookes came to his radiometer, first without and then with electrification; and, powerfully aided by Sir George Stokes,‡ he brought all his work more and more into touch with the kinetic theory of gases; so much so that when he discovered the molecular torrent he immediately gave it its true explanation—molecules of residual air, or gas, or vapour projected at great velocities§ by electric repulsion from the negative electrode. This explanation has been repeatedly and strenuously attacked by many other able investigators, but Crookes has defended|| it, and thoroughly established it by what I believe is irrefragable evidence of experiment. Skilful investigation perseveringly continued brought out more and more of wonderful and valuable results: the non-importance of the position of the positive electrode; the projection of the torrent *perpendicularly* from the surface of the negative electrode; its convergence to a focus and divergence thenceforward when the surface is slightly convex; the slight but perceptible repulsion between two parallel torrents due, according to Crookes, to negative electrifications of their constituent molecules; the change of direction of the molecular torrent by a neighbouring magnet; the tremendous heating effect of the torrent from a concave electrode when glass, metal, or any ponderable substance is placed in the focus; the phosphorescence produced on a plate coated with sensitive paint by a molecular torrent skirting along it; the brilliant colours—turquoise-blue, emerald, orange, ruby-red—with which grey colourless objects and clear colourless crystals glow on their struck faces when lying separately or piled up in a heap in the course of a molecular torrent; "electrical evaporation" of negatively electrified liquids and solids;¶ the seemingly red hot glow, but with no heat conducted inwards from the surface, of cool solid silver kept negatively electrified in a vacuum of 1/1,000,000 of an atmosphere, and thereby caused

* Tribulation, not undisturbed progress, gives life and soul, and leads to success when success can be reached, in the struggle for natural knowledge.

† Crookes, "On the Viscosity of Gases at High Exhaustions,' § 655, 'Phil. Trans.,' Feb., 1881, p. 403.

‡ 'Phil. Trans.,' vol. 172 (1881), pp. 387, 435.

§ Probably, I believe, not greater in any case than 2 or 3 kilometres per second.

|| Address to the Institute of Telegraphic Engineers, 1891.

¶ 'Roy. Soc. Proc.,' June 11, 1891.

to rapidly evaporate. This last-mentioned result is almost more surprising than the phosphorescent glow excited by molecular impacts in bodies not rendered perceptibly phosphorescent by light. Both phenomena will surely be found very telling in respect to the molecular constitution of matter and the origination of thermal radiation, whether visible as light or not. In the whole train of Crookes' investigations on the radiometer, the viscosity of gases at high exhaustions, and the electric phenomena of high vacuums, ether seems to have nothing to do except the humble function of showing to our eyes something of what the atoms and molecules are doing. The same confession of ignorance must be made with reference to the subject dealt with in the important researches of Schuster and J. J. Thomson on the passage of electricity through gases. Even in Thomson's beautiful experiments showing currents produced by circuital electromagnetic induction in complete poleless circuits, the presence of molecules of residual gas or vapour seems to be *the essential.* It seems certainly true that without the molecules there could be no current, and that without the molecules electricity has no meaning. But in obedience to logic I must withdraw one expression I have used. We must not imagine that "presence of molecules is *the* essential." It is certainly *an* essential. Ether also is certainly *an* essential, and certainly has more to do than merely to telegraph to our eyes to tell us of what the molecules and atoms are about. If a first step towards understanding the relations between ether and ponderable matter is to be made, it seems to me that the most hopeful foundation for it is knowledge derived from experiment on electricity in high vacuum; and if, as I believe is true, there is good reason for hoping to see this step made, we owe a debt of gratitude to the able and persevering workers of the last forty years who have given us the knowledge we have: and we may hope for more and more from some of themselves and from others encouraged by the fruitfulness of their labours to persevere in the work.

The President then presented the Medals awarded by the Society as follows:—

Copley Medal.

Sir G. Gabriel Stokes, Bart., F.R.S.

In presenting the Copley Medal to Sir George Stokes I feel that no "statement of claim" is needed. Nevertheless, it is interesting to recall to memory something of the great work that he has done in mathematical and physical science. Fifty-two years ago he took up the subject of fluid motion with mathematical power amply capable to advance on the lines of Lagrange, Fourier, Cauchy, Poisson, in the

splendid nineteenth century "physical mathematics," invented and founded by those great men; and with a wholly original genius for discovery in properties of real matter, which enhanced the superlative beauty of the mathematical problems by fresh views deep into the constitution of matter.

In the purely mathematical part of his hydro-dynamical subject he advanced from the "infinitely small" waves of Cauchy and Poisson to deep-sea waves of such considerable steepnesses and lengths and heights as are seen in nature—on water 500 fathoms deep, or more, after a severe gale far away from land; and he has shown how to carry on his mode of solution right up to breaking waves at sea and tidal bores in shallow water.

His enunciations and solutions for motion of viscous fluids, rich with applications to natural phenomena—the distance of audibility of sound, the suspension of clouds in the air, the subsidence of ripples on a pond and of waves on the ocean after the cessation of wind; and rich in aids to scientific investigation, as in the theory of the pendulum in air—have added to hydro-dynamics a previously unknown province, in which the exceeding difficulty of the mathematical work deserves every capable effort to advance it, on account of the vast practical and scientific importance of the issues.

His "instability" of the motion of a viscous fluid in the neighbourhood of a solid gives the key to the scientific mystery of turbulent motion in practical hydraulics, and in wind blowing over solid earth; the hearing of sounds in the direction of the wind which are inaudible at equal distances in the contrary direction; the flow of water in rivers, culverts, and water supply pipes; the "skin-resistance" of ships, the scientific consideration of which has done so much to make 22 knots a proper speed for travelling by sea.

Of true dynamical science in all these subjects Stokes' early work was the beginning. It also first gave true views as to that very important practical subject, the rigidity, and the resistance against compression, of solids; views which would be false if a majority of votes in the scientific world of 1893 could decide between truth and error.

In optics and the undulatory theory of light, Stokes has been the teacher and guide of his contemporaries. His Report to the British Association in 1862, "On Double Refraction," showed with perfect accuracy and clearness the outstanding difficulties, but called special attention to the door which Green had opened for escape from them. That Report has given the starting impulse and essential information for nearly all that has been done for the subject in England since its publication.

By his own experimental and mathematical work on the polarisation of light by a grating, and of the light of the blue sky; by his

experimental investigation of "epipolic dispersion" (or "fluorescence"), and, perhaps more than all, by his accurate *measuring* work, from which he drew an exceedingly rigorous verification of the accuracy of Huyghens' geometrical construction for the double refraction of Iceland spar, Sir George Stokes has done much to make the Undulatory Theory of Light sure and strong as it is—a codification of laws divined by Huyghens and Fresnel. But he has done more than this. He has not merely left to mathematicians and speculative physicists a desperate problem to find the dynamical explanation of those laws. He has given (perhaps only in conversation) what seems to me certainly the true clue to the dynamics of the Undulatory Theory of Light by pointing out that we must look not merely, or not at all, to change of shape of the portion of ether within a wave-length in the motion constituting light, but also, or altogether, to its absolute rotation, for explaining the efficient force.

Royal Medal.

Professor Arthur Schuster, F.R.S.

Professor Schuster's first researches in physics were with the spectroscope. Some of these date more than ten years ago. He has been an important observer in more than one Solar Eclipse Expedition. The results he has obtained will be found in joint papers in the 'Phil. Trans.,' 1884 and 1889. He was associated with Lord Rayleigh in one of his determinations of the ohm. In 1889 also he showed that the diurnal magnetic changes could be accounted for by a disturbing cause outside the earth's surface, and could not be accounted for by a disturbing cause within the earth's surface.

But perhaps the researches of most general interest are those on electric discharge through gases. These will be found in the "Proceedings of the Royal Society," vols 42 and 47. In the former paper he shows, amongst other things, that a steady current of electricity can be obtained in air from electrodes at the ordinary temperature, which are at a difference of potential of a quarter of a volt only, provided that an independent current is maintained in the same closed vessel. A vessel from which the air could be exhausted was nearly divided into two parts by a metallic partition connected to earth. A pair of electrodes was introduced on one side of the partition between which a current could be passed from a battery of many cells. A second pair was introduced into the vessel on the other side of the partition, and these were connected to the poles of a battery of low electromotive force. When high electric pressure was applied to produce a current between the first pair of electrodes, a current due to the battery of low electromotive force also passed between the second pair. Professor Schuster supposes that the effect

of the first current of high electromotive force is to break up some of the molecules of gas, and that the wandering atoms can then yield to small electrical forces to which the complete molecule is insensitive.

Professor Schuster has worked in many branches of physics, and has advanced the knowledge of each.

Royal Medal.

Professor H. Marshall Ward, F.R.S.

Professor H. Marshall Ward's claims to the recognition by the Royal Society implied in the award of a Royal Medal rest mainly on the series of brilliant investigations he has carried out, having for their object the elucidation of the phenomena of life exhibited by the Fungi and the Schizomycetes, and the effects of the life of these plants, whether exhibited in a hurtful form as disease in living organisms, or in an useful form in the production of substances of economic importance. These investigations, planned with ingenuity, and completed with resourceful industry, have resulted in most valuable contributions to natural knowledge. In the additions they make to the sum of our knowledge regarding the plant forms dealt with they are admirable and weighty, but, over and above the botanical interest attaching to the work, it has a far-reaching importance in the whole world of biology, touching as it does the fundamental point of the interaction of living organisms and the conditions under which symbiosis, whether beneficial or baneful, is developed and maintained; and its conclusions are of vital value in their immediate bearing upon such large industries as agriculture, horticulture, forestry, and brewing, and in relation to the scientific basis of sanitary procedure.

The series of purposeful and practical researches which place Professor Marshall Ward in the front rank of biologists was initiated by his investigation of the disease which devastated, a decade or so ago, the coffee plantations in Ceylon. Undertaking this work at the invitation of the Ceylon Government, Professor Marshall Ward exposed, in a detailed account of the life history of *Hemileia vastatrix*, the fungus which immediately caused the disease, and whilst his work contributed many new morphological and physiological facts, its chief importance lay in the scientific basis it established, upon which any method of treatment that might be adopted should be founded. Following up the line thus entered upon, Professor Marshall Ward took up the better known questions of the salmon disease, the damping off of seedling plants, and the potato disease, and, in an admirably recorded series of observations upon the Fungi connected with these diseases, brought to light many points of great importance, especially in the direction of showing the influences

exercised by the host in modifying the structure of the parasite, and conversely the effect of the parasite upon the host, and the mechanism of its attack. Subsequent work on the disease of lilies is specially valuable, on account of the recognition and separation of the ferment by which the passage of fungi through the tissues of their host is effected. Upon the recrudescence of the question of the relation of plants to the free nitrogen of their environment, which we owe to the experimental and structural investigations in connexion with leguminous crops, carried on in this country at Rothamsted, and in Germany by Frank and Hellriegel, we find Professor Marshall Ward attacking the point from his practical standpoint, and to him we are indebted for the discovery that the well-known nitrogen-storing root tubercles of the Leguminosæ are a consequence of the attack of a fungus, the mycelium of which penetrates the root hairs, and gives origin eventually to the active "bacteroids" in the tubercle, a fundamental fact in the relation of the plants to the soil.

The best summary of the general bearing of his several investigations up to date is probably that provided by himself in the Croonian Lecture he delivered, in 1890, upon the relations between host and parasite in plant diseases, one of the most suggestive of the many interesting lectures that have been given under the foundation, and it is not amiss to mention here also, as remarkable for its suggestiveness, his paper of earlier date on "Sexuality in Fungi," in which the progressive apogamy of the group is dealt with, and the parasitic habit laid under contribution for an explanation.

Of the many valuable pieces of work done by Professor Marshall Ward, not the least remarkable is that on the ginger-beer plant—a model of experimental biological investigation—in which the remarkable symbiosis of yeast and bacteria is unfolded, and the idea, pregnant for industries such as brewing, of symbiotic as distinct from metabiotic and antibiotic, fermentation is put forward.

In the Reports, in conjunction with Professor Frankland, upon bacteria in water, now being presented to the Society, we have Professor Marshall Ward's most recent work, and, in connexion with it, his probing of the question of the action of light in arresting the development of and killing bacteria—to mention only one of the many points raised in the Reports—has already brought out striking results, the significance of which, from a sanitary point of view, is sufficiently apparent, and it has led to other investigations by the author into the wide question of the function of colour in the vegetable kingdom, which promise to be fruitful in valuable generalisations.

In the field of work to which I have referred, one of no ordinary difficulty, Professor Marshall Ward has not merely increased enormously our stock of knowledge of solid fact, but in his dealing with facts under review he has thrown many lights upon general

biological problems, and has, moreover, opened up many new lines of research of immense interest in relation to economic questions.

The Davy Medal

is awarded in duplicate to J. H. van't Hoff and J. A. Le Bel, in recognition of their important services to theoretical chemistry.

J. H. van't Hoff was the first to introduce a definite mechanical conception of valency, and to connect the optical chirality displayed by many carbon compounds with their chemical constitution. The fact that such compounds always contain at least one atom of carbon combined in four different ways has now been fully established, and the doctrine of "asymmetric" results from the quadrivalence of carbon forms the basis of the modern theory of stereo-isomerism.

Van't Hoff's first paper upon the subject was published in September, 1874, in the Dutch language, but appeared in 1875 under the title "La Chimie dans l'Espace."

At that time many compounds were known which seemed to constitute exceptions to the new generalisation, but further investigation has shown that these exceptions were only apparent, and are now included in and are explained by the theory.

A second very important contribution to science resulted from the mathematical investigations of van't Hoff in reference to the properties of liquids and the condition of dissolved solids in dilute solutions. By these investigations he has supplied the theoretical explanation of the phenomena involved in the method of determining molecular weights, which we owe to the experimental investigations of Professor Raoult, and for which he received the Davy Medal last year.

To M. J. A. Le Bel we owe an independent enunciation of the theory of asymmetric combinations with carbon, published in the 'Bull. de la Soc. Chim.' only two months later than the Dutch paper of van't Hoff, that is, in November, 1874. Le Bel has contributed substantially by his work on active amyl alcohol and its derivatives to the support of the theory which, as already stated, is accepted universally by chemists. The justification for what appears at first a purely speculative hypothesis is supplied by the fact that the introduction of the new views has led to the discovery of many new carbon compounds the existence of which could not have been previously suspected.

The Statutes relating to the election of Council and Officers were then read, and Mr. Crookes and Admiral Sir Erasmus Ommanney, having been, with the consent of the Society, nominated Scrutators, the votes of the Fellows present were taken, and the following were declared duly elected as Council and Officers for the ensuing year:—

President.—The Lord Kelvin, D.C.L., LL.D.

Treasurer.—Sir John Evans, K.C.B., D.C.L., LL.D.

Secretaries.— { Professor Michael Foster, M.A., M.D.
The Lord Rayleigh, M.A., D.C.L.

Foreign Secretary.—Sir Joseph Lister, Bart., F.R.C.S.

Other Members of the Council.

Professor Isaac Bayley Balfour, M.A.; Andrew Ainslie Common, LL.D.; Andrew Russell Forsyth, Sc.D.; Richard Tetley Glazebrook, M.A.; Professor Alexander Henry Green, M.A.; Sir John Kirk, K.C.B.; Professor Oliver Joseph Lodge, D.Sc.; Sir John Lubbock, Bart., D.C.L.; William Davidson Niven, M.A.; William Henry Perkin, LL.D.; the Marquis of Salisbury, K.G., M.A.; Professor J. S. Burdon Sanderson, M.D.; Adam Sedgwick, M.A.; Professor Thomas Edward Thorpe, Sc.D.; Professor William Augustus Tilden, D.Sc.; Professor W. Cawthorne Unwin, B.Sc.

The thanks of the Society were given to the Scrutators.

Balance Sheet. 1893.

***Statement of Receipts and Expenditure from November* 12th, 1892, *to November* 11th, 1893.**

	£	s.	d.	£	s.	d.
To Balance at Bank, 12th November, 1892				1,125	10	4
,, Balance in hand, Catalogue Account	19	4	1	33	1	7
,, ,, Petty Cash	13	17	6			
,, Compositions				120	0	0
,, Admission Fees				20	0	0
,, Annual Contributions, at £4	£504	0	0	1,053	0	0
,, at £3	549	0	0			
,, Fee Reduction Fund, in lieu of Admission Fees and Annual Contributions				331	0	0
,, Rents:						
Fee Farm, Lewes	18	12	10	100	0	6
Mablethorpe Estate	81	7	8			
,, Ground Rents				608	6	2
,, Dividends (exclusive of Trust Funds)				2,291	5	10
,, Interest on Mortgage Loans (Duke of Norfolk)				313	14	4
,, Sale of Transactions and Proceedings				649	6	0
Catalogue of Scientific Papers (Mr. Ludwig Mond's Donation)				2,000	0	0
,, Interest on Bank Deposit Account				21	19	11
,, Interest on Bank Deposit (Catalogue Account)				10	2	6
,, Sale of Catalogue				59	14	3
,, Transfer from Handley Fund on account of Catalogue				219	11	5
,, Sale of Krakatoa Report (leaving £62 10*s*. 4*d*. Expenditure in excess of Receipts)				9	14	10
,, Sale of Lendenfeld Monograph (leaving £664 19*s*. 1*d*. expenditure in excess of receipts)				....	....	....
,, Water Research Grants (leaving £250 further now due from London County Council)				400	0	0
,, Income Tax refunded				818	0	10
,, Proceeds of Sale of £2,128 9*s*. 6*d*. Bank Stock and £4,914 8*s*. 4*d*. Consols				12,000	0	0
,, Bequest (Earl of Derby)				2,000	0	0
				£24,184	8	6

	£	s.	d.	£	s.	d.
By Salaries, Wages, and Pension				1,632	10	6
,, Catalogue of Scientific Papers				376	7	3
,, Books for the Library				306	11	5
,, Printing and Advertising Transactions, and Separate Copies to Authors and Publisher	898	0	5	3,172	10	7
,, Ditto Proceedings, Nos. 316 to 326	610	14	6			
,, Ditto Miscellaneous	130	11	0			
,, Paper for Transactions and Proceedings	511	12	3			
,, Binding ditto	44	11	6			
,, Engraving and Lithography	977	0	11			
,, Soirée and Reception Expenses	117	18	5	162	9	11
,, Anniversary Expenses	44	11	6			
,, Coal, Lighting, &c.	163	18	3	1,053	4	0
,, Office Expenses	44	17	3			
,, House Expenses	557	18	11			
,, Tea Expenses	15	16	8			
,, Fire Insurance	55	5	0			
,, Taxes	46	2	6			
,, Advertising Meetings	17	13	6			
,, Postage, Parcels, and Petty Charges	80	7	2			
,, Miscellaneous Expenses	71	4	9			
,, Carrington Donation				30	0	0
,, Water Research, Payments				364	15	0
,, Purchase of £2,022 14*s*. 1*d*. India 3 per cent. Stock				2,000	0	0
,, Kew Committee Loan				200	0	0
,, Loan on Mortgage, Duke of Norfolk				12,000	0	0
,, Balance at Bankers				2,875	16	6
Including £500 "Challenger" Account and £2,200 Catalogue Account.						
,, Balance on hand, Catalogue Account	3	3	3	10	3	4
,, Ditto, Petty Cash	7	0	1			
				£24,184	8	6

Trust Funds.

	£	s.	d.	£	s.	d.
To Balance at Bank 12th November, 1892:—						
General Account	2,032	9	1			
Fee Reduction Fund Account	307	18	1	3,938	12	3
Scientific Relief Fund Account	1,533	4	4			
Joule Memorial Fund Account	65	0	9			
„ Scientific Relief Fund, Dividends, &c.	767	1	10			
„ Donation Fund, Dividends, and Transfer from Jodrell Fund, &c.	434	3	8			
„ Rumford Fund, Dividends, &c.	74	10	11			
„ Bakerian and Copley Medal Fund, Dividends, &c.	64	18	7			
„ Keck Bequest, Dividends, &c.	27	11	1			
„ Wintringham Fund, Dividends, &c.	38	7	4			
„ Croonian Lecture Fund, Rent, &c.	102	3	2			
„ Davy Medal Fund, Dividends, &c.	37	15	9	3,316	8	8
„ Gassiot Trust, Dividends, &c.	586	11	1			
„ Handley Fund, Dividends, &c.	219	11	5			
„ Jodrell Fund, Dividends	138	12	7			
„ Fee Reduction Fund, Dividends, &c.	504	13	9			
„ Darwin Memorial Fund, Dividends, &c.	99	0	5			
„ Joule Memorial Fund, Dividends, &c.	197	8	4			
„ Brady Library Account, Interest	3	18	9			
„ Gunning Fund, Interest	20	0	0			
				£7,255	0	11

	£	s.	d.	£	s.	d.
By Scientific Relief Fund, Grants and Investments	2,009	9	4			
„ Donation Fund, Grants	538	17	7			
„ Rumford Fund Medals	59	11	6			
„ Bakerian and Copley Medal Fund, Medal, Lecture, and Gifts	108	12	7			
„ Keck Bequest, Payment to Foreign Secretary	23	7	6			
„ Wintringham Fund, Payment to Foundling Hospital	32	4	0			
„ Croonian Lecture Fund Payments	100	0	0			
„ Davy Medal Fund, Gold Medals	32	8	0	4,585	1	6
„ Gassiot Trust, Payments to Kew Committee	486	9	2			
„ Handley Fund, Transfer to General Account, for Catalogue	219	11	5			
„ Jodrell Fund, Transfer to Donation Fund	138	12	7			
„ Fee Reduction Fund, Transfer to Royal Society General Account and Investment	669	5	6			
„ Darwin Memorial Fund, Silver Medal	1	5	0			
„ Joule Memorial Fund, Investment	165	7	4			
„ Balance at Bankers:—						
General Account	2,138	14	6			
Fee Reduction Fund Account	143	6	4	2,669	19	5
Scientific Relief Fund Account	290	16	10			
Joule Memorial Fund Account	97	1	9			
				£7,255	0	11

Estates and Property of the Royal Society, including Trust Funds.

Estate at Mablethorpe, Lincolnshire (55A. 2R. 2P.), rent £85 per annum.
Ground Rent of House, No. 57, Basinghall Street, rent £380 per annum.
„ „ of 23 houses in Wharton Road, West Kensington, rents £253 per annum.
Fee Farm Rent, near Lewes, Sussex, £19 4*s.* per annum.
One-fifth of the clear rent of an estate at Lambeth Hill, from the College of Physicians, about £52 per annum, Croonian Lecture Fund.
Stevenson Bequest. Chancery Dividend. One-fourth annual interest on balance of Bequest still in Court.
£15,200 Mortgage Loan, $3\frac{1}{2}$ per Cent., to the Duke of Norfolk.

£14,568 0*s.* 4*d.*, $2\frac{3}{4}$ per Cent. Consolidated Stock, being £10,779 8*s.* 2*d.* on account of the following Funds:—

	£	*s.*	*d.*
Rumford Fund	2,330	0	0
Wintringham Fund	1,200	0	0
Gassiot Trust	400	0	0
Sir J. Copley Fund	1,666	13	4
Jodrell Fund	5,182	14	10

£270 11*s.* 11*d.* General Purposes.
and £3,518 0*s.* 3*d.* in Chancery, arising from sale of the Coleman Street Estate.—General Purposes.

£913 0*s.* 3*d.* New $2\frac{1}{2}$ per cent. Stock: £403 9*s.* 8*d.* Bakerian and Copley Medal Fund; £509 10*s.* 7*d.* Scientific Relief Fund.
£3,000 India $3\frac{1}{2}$ per Cent. Stock.—General Purposes.
£2,022 14*s.* 1*d.* India 3 per Cent. Stock.—General Purposes (Earl of Derby's Bequest).
£800 Midland Railway 3 per Cent. Debenture Stock.—Keck Bequest.
£370 3*s.* 7*d.* Midland Railway 4 per Cent. Perpetual Guaranteed Preference Stock.—General Purposes (Stevenson Bequest).
£5,660 Madras Railway Guaranteed 5 per Cent. Stock: General Purposes, £5,000; Davy Medal Fund, £660.
£10,000 Italian Irrigation (Cavour Canal) Bonds.—The Gassiot Trust.
£9,061 6*s.* 8*d.* Great Northern Railway 3 per Cent. Debenture Stock: Scientific Relief Fund, £7,200; The Trevelyan Bequest, £1,861 6*s.* 8*d.*
£5,030 Great Northern Railway Perpetual 4 per Cent. Guaranteed Stock.—Donation Fund.
£2,725 „ „ „ 4 per Cent. Perpetual Preference Stock.—General Purposes (Stevenson Bequest).
£4,700 Metropolitan $3\frac{1}{2}$ per Cent. Stock.—Fee Reduction Fund.
£7,000 London and North Western Railway 4 per Cent. Perpetual Debenture Stock.—Fee Reduction Fund.
£20,908 „ „ „ „ 4 per Cent. Consolidated Guaranteed Stock.—£6,000 Scientific Relief Fund; £12,150 General Purposes; £2,758 „ „ (Stevenson Bequest).
£5,000 „ „ „ „ Consolidated 4 per Cent. Preference Stock.—General Purposes.

£5,000 North Eastern Railway 4 per Cent. Preference Stock.—General Purposes.
£2,760 " " " Consolidated 4 per Cent. Guaranteed Stock.—General Purposes (Stevenson Bequest).
£2,200 South Eastern Railway 4 per Cent. Debenture Stock.—Darwin Memorial Fund.
£4,340 South Eastern Railway 5 per Cent. Debenture Stock.—Scientific Relief Fund.
£3,333 London and South Western Railway 4 per Cent. Preference Stock.—General Purposes.
£4,798 Lancashire and Yorkshire Railway 4 per Cent. Guaranteed Stock.—Handley Fund.
£900 London, Brighton, and South Coast Railway Consolidated Guaranteed 5 per Cent. Stock.—Joule Memorial Fund.
£4,000 Southern Mahratta Railway 4 per Cent. Debenture Stock.—General Purposes.
£307 8*s.* 6*d.* on Deposit Account at Bank.—Brady Library Account.
£150 on Deposit Account on behalf of the Committee.—Joule Memorial Fund.
£2,000 on Deposit Account at Bank, Mr. Ludwig Mond's Gift.—Catalogue Account.
£1,000 Policy in the Atlas Assurance Office, becoming due October 7th, 1899, No. 24644.—Catalogue Account.
£1,000 Bond.—Dr. Gunning.—Interest to be applied to the promotion of Physics and Biology.

JOHN EVANS, *Treasurer.*

We, the Auditors of the Treasurer's Accounts on the part of the Council, have examined these Accounts and found them correct.

B. BAKER.
M. FOSTER.
F. D. GODMAN.

We, the Auditors of the Treasurer's Accounts on the part of the Society, have examined these Accounts and found them correct.

A. H. CHURCH.
W. C. ROBERTS-AUSTEN.

Trust Funds. 1893.

Scientific Relief Fund.

£6,000 L. & N.W.R. 4 per Cent. Consolidated Guaranteed Stock.
£7,200 Great Northern Railway 3 per Cent. Debenture Stock.
£4,340 South Eastern Railway 5 per Cent. Debenture Stock.
£509 10*s.* 7*d.* 2½ per Cent. Annuities.

Dr.

	£	*s.*	*d.*	£	*s.*	*d.*
To Balance { Income	1,047	14	7			
To Balance { Capital	485	9	9			
				1,533	4	4
,, Dividends				664	15	7
,, Interest on Deposit				7	17	3
,, Return of Income Tax				94	9	0
				£2,300	6	2

Cr.

				£	*s.*	*d.*
By Grants				952	0	0
,, Purchase of £533 6*s.* 8*d.* Great Northern Railway 3 per Cent. Debenture Stock	£557	9	4			
,, Purchase of £509 10*s.* 7*d.* 2½ per Cent. Annuities	500	0	0			
				1,057	9	4
,, Balance, Income	£862	16	5			
,, Less Capital over-invested	571	19	7			
				290	16	10
				£2,300	6	2

Donation Fund.

£5,030 Great Northern Railway Perpetual 4 per Cent. Guaranteed Stock.
The Trevelyan Bequest. £1,861 6*s.* 8*d.* Great Northern Railway 3 per Cent. Debenture Stock.

	£	*s.*	*d.*
To Balance	746	13	1
,, Dividends	250	6	10
,, Transfer from Jodrell Fund	138	12	7
,, Return of Income Tax	45	4	3
	£1,180	16	9

	£	*s.*	*d.*
By Grants	538	17	7
,, Balance	641	19	2
	£1,180	16	9

Rumford Fund.

£2,330 $2\frac{3}{4}$ per Cent. Consolidated Stock.

	£	s.	d.		£	s.	d.
To Balance	208	8	2	By Medals	59	11	6
,, Dividends	62	7	3	,, Balance	223	7	7
,, Return of Income Tax	12	3	8				
	£282	19	1		£282	19	1

Bakerian and Copley Medal Fund.

Sir Joseph Copley's Gift. £1,666 13*s.* 4*d.* $2\frac{3}{4}$ per Cent. Consolidated Stock.
£403 9*s.* 8*d.* New $2\frac{1}{2}$ per Cent. Stock.

	£	s.	d.		£	s.	d.
To Balance	156	18	6	By Gold Medal	4	12	7
,, Dividends, New $2\frac{1}{2}$ per Cent. Stock	9	16	2	,, Bakerian Lecture, Professor H. B. Dixon	4	0	0
,, Dividend—Sir J. Copley's Fund	44	11	8	,, Gifts:—			
,, Return of Income Tax	10	10	9	Professor S. Cannizzaro	50	0	0
				Professor R. Virchow	50	0	0
				,, Balance	113	4	6
	£221	17	1		£221	17	1

The Keck Bequest.

£800 Midland Railway 3 per Cent. Debenture Stock.

	£	s.	d.		£	s.	d.
To Dividends	23	7	6	By Payment to Foreign Secretary	23	7	6
,, Return of Income Tax	4	3	7	,, Balance	4	3	7
	£27	11	1		£27	11	1

Wintringham Fund.

£1,200 $2\frac{3}{4}$ per Cent. Consolidated Stock.

	£	s.	d.		£	s.	d.
To Balance	32	4	0	By Payment to Foundling Hospital	32	4	0
,, Dividends	32	2	10	,, Balance	38	7	4
,, Return of Income Tax	6	4	6				
	£70	11	4		£70	11	4

Croonian Lecture Fund.

One-fifth of the clear rent of an Estate at Lambeth Hill, from the College of Physicians, about £52 per annum.

	£	s.	d.		£	s.	d.
To Rent	101	5	10	By Lecture (1892) Professor Mosso	50	0	0
,, Return of Income Tax		17	4	,, (1893) Professor Virchow	50	0	0
				,, Balance	2	3	2
	£102	3	2		£102	3	2

Davy Medal Fund.

£660 Madras Railway Guaranteed 5 per Cent. Stock.

	£	s.	d.		£	s.	d.
To Balance	74	6	10	By Gold Medals	32	8	0
,, Dividends	32	2	1	,, Balance	79	14	7
,, Return of Income Tax	5	13	8				
	£112	2	7		£112	2	7

The Gassiot Trust.

£10,000 Italian Irrigation Bonds.
£400 $2\frac{3}{4}$ per Cent. Consolidated Stock.

	£	s.	d.		£	s.	d.
To Balance	90	14	9	By Payments to Kew Committee	486	9	2
,, Dividends	497	3	5	,, Balance	190	16	8
,, Return of Income Tax	89	7	8				
	£677	5	10		£677	5	10

Handley Fund.

£4,798 Lancashire and Yorkshire Railway 4 per Cent. Guaranteed Stock.

	£	s.	d.		£	s.	d.
To Dividends	186	18	4	By Transfer to Catalogue Account	219	11	5
,, Return of Income Tax	32	13	1				
	£219	11	5		£219	11	5

The Jodrell Fund.

£5,182 14*s.* 10*d.* $2\frac{3}{4}$ per Cent. Consolidated Stock.

	£	s.	d.		£	s.	d.
To Dividends	138	12	7	By Transfer to Donation Fund	138	12	7

Fee Reduction Fund.

£4,700 Metropolitan $3\frac{1}{2}$ per Cent. Stock.
£7,000 London and North Western Railway 4 per Cent. Perpetual Debenture Stock.

	£	s.	d.		£	s.	d.
To Balance	307	18	1	By Transfer to Royal Society General Account	331	0	0
,, Dividends	430	4	1	,, Purchase of £300 Metropolitan $3\frac{1}{2}$ per Cent. Stock	338	5	6
,, Return of Income Tax	74	9	8	,, Balance	143	6	4
	£812	11	10		£812	11	10

Darwin Memorial Fund.

£2,200 South Eastern Railway 4 per Cent. Debenture Stock.

	£	s.	d.		£	s.	d.
To Balance	379	14	0	By Silver Medal	1	5	0
,, Dividends	85	14	3	,, Balance	477	9	5
,, Return of Income Tax	13	6	2				
	£478	14	5		£478	14	5

Joule Memorial Fund.

£900 London, Brighton, and South Coast Railway Consolidated Guaranteed 5 per Cent. Stock.
£150 on Deposit on behalf of the Committee.

	£	s.	d.		£	s.	d.
To Balance	65	0	9	By Purchase of £100 London, Brighton and South Coast Railway Stock	165	7	4
,, Interest on Deposit	2	3	3	,, Balance	97	1	9
,, Dividends	43	16	7				
,, Return of Income Tax	1	8	6				
,, Amount withdrawn from Deposit	150	0	0				
	£262	9	1		£262	9	1

Brady Library Fund.

£307 8*s.* 6*d.* on Deposit Account at Bank.

	£	s.	d.		£	s.	d.
To Amount on Deposit at Bank	303	9	9	By Balance on Deposit	307	8	6
,, Interest thereon	3	18	9				
	£307	8	6		£307	8	6

Gunning Fund.

£1,000 4 per Cent. Bond of His Excellency Dr. Gunning.

	£	s.	d.
To Balance	40	0	0
„ Interest	20	0	0
	£60	0	0

	£	s.	d.
By Balance	60	0	0
	£60	0	0

Income and Expenditure Account from 13*th November*, 1892, *to* 11*th November*, 1893.

RECEIPTS.

	£	s.	d.	£	s.	d.
To Balance at Bank and in hand 12th November, 1892	1,158	11	11			
„ *Less* Catalogue and "Challenger" Account	700	0	0			
				458	11	11
„ Composition and Admission Fees				140	0	0
„ Annual Contributions				1,384	0	0
„ Rents				708	6	8
„ Dividends and Interest				2,856	14	0
„ Sale of Publications				718	15	1
„ Income Tax refunded				818	0	10
„ Water Research Grants				400	0	0
				£7,484	8	6

EXPENDITURE.

	£	s.	d.	£	s.	d.
By Salaries, Wages, and Pension				1,632	10	6
„ Publications				3,548	17	10
„ Books for the Library				306	11	5
„ Miscellaneous Expenses, Office, House, Receptions, &c.				1,245	13	11
„ Water Research Payments				364	15	0
„ Kew Committee Loan				200	0	0
„ Balance at Bank and in hand	2,885	19	10			
Less Catalogue and "Challenger" Account	2,700	0	0			
				185	19	10
				£7,484	8	6

The following Table shows the progress and present state of the Society with respect to the number of Fellows :—

	Patron and Royal.	Foreign.	Compounders.	£4 yearly.	£3 yearly.	Total.
Nov. 30, 1892 ..	4	50	156	127	170	507
Since Elected ..	+ 1		+ 1	+ 1	+ 14	+ 17
Since Deceased ..		− 2	− 4	− 4	− 3	− 13
Since Compounded						
Nov. 30, 1893 ..	5	48	153	124	181	511

Account of Grants from the Donation Fund in 1892–93.

	£	s.	d.
Prof. G. H. Darwin, for assistance in the Reduction of Tidal Observations	25	0	0
Sir A. Geikie, to assist a Committee appointed by the British Association in an Investigation of the character of certain High Level Shell Beds in Scotland	30	0	0
Prof. M. Foster, to aid Dr. Edkins in his Researches on Absorption from the Alimentary Canal..............	30	0	0
Prof. W. C. Williamson, in aid of his Researches on Fossil Plants	30	0	0
Prof. J. Milne, in aid of his Seismographic Investigations	100	0	0
Sir A. Geikie, for the purchase of a Microscope to aid Dr. Platania in his Mineralogical Researches	20	0	0
Prof. Schäfer, in aid of his Researches on Cerebral Localisation....................................	50	0	0
Prof. Roy, in aid of his Researches on the Physiology and Pathology of the Circulation of the Blood............	50	0	0
Sir W. H. Flower, to assist Mr. Lydekker in the Examination of Fossil Remains in La Plata	120	0	0
Prof. Fleming, in aid of his Researches on Temperature Changes in Electrical Resistance of Pure Metals and Alloys over Wide Ranges of Temperature............	50	0	0
Dr. A. Harden and Dr. Dyson, for a Research on the Combination of Chlorine with other Gases under the Influence of Light	20	0	0
Carried forward..........	£525	0	0

	£	s.	d.
Brought forward..........	525	0	0
W. T. Thiselton Dyer, to aid Mr. Theodore Bent in making Botanical Collections in Southern Arabia......	50	0	0
Prof. McKendrick, to aid Mr. Jack in Researches on Muscular Movements..............................	15	0	0
	£590	0	0
Repayments...............	51	2	5
	£538	17	7

"Alternate Current Electrolysis." By J. HOPKINSON, D.Sc., F.R.S., E. WILSON, and F. LYDALL. Received November 2, —Read November 23, 1893.

Our attention has been called to the interesting work of Messrs. Bedell, Ballantyne, and Williamson on "Alternate Current Condensers and Dielectric Hysteresis," in the 'Physical Review' for September—October, 1893. As experiments bearing upon an analogous subject were carried out in the Siemens Laboratory, King's College, London, we think it may be of interest to publish them. Our experiments were commenced in June, 1892, and were discontinued in the following July with the intention of resuming them at a future time. They are therefore not exhaustive.

Suppose an alternating current to be passed through an electrolyte between electrodes, and that the current passing and the difference of potential are measured at intervals during the phase. If the electrolytic action were perfectly reversible, we should expect to find the potential difference to have its maximum value when the current was zero, that is to say, when the total quantity of electricity had also a maximum value. One object we had in view was to ascertain if this were the case, and, if not, to determine what amount of energy was dissipated under different conditions.

This is readily done, inasmuch as the work done on the voltameter or by the voltameter in any short time is the total quantity of current passed in the time multiplied by the potential difference. Let a curve be drawn in which the ordinates are the coulombs and the abscissæ the volts at corresponding times: the area of this curve represents the work dissipated in a cycle.

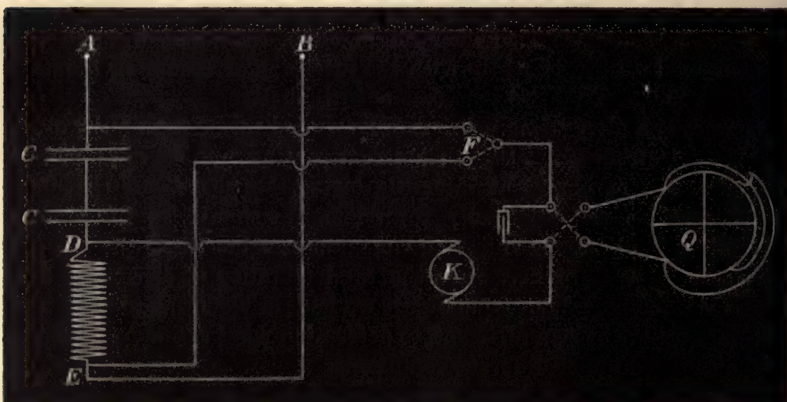
It is, of course, well known that if a current is passed through an electrolyte, the potential difference speedily attains a certain maximum value and there remains. If an alternate current is passed, we should expect to find that as the number of coulombs passed in each

half period increased, the potential difference would also increase, until it attained the value given with a continuous current, and that when this value was attained, the curve of potential and time would exhibit a flat top for all higher numbers of coulombs passed. We thought it possible that from the number of coulombs per unit of section required to bring the potential difference to its full value, we could obtain an idea of how thick a coating of the ions sufficed to secure that the surface of the plate had the chemical quality of the ion and not of the substance of the plate.

Platinum Plates.

Part I.—In the first instance, two cells having platinum plates for electrodes were used. We are indebted to Messrs. Johnson and Matthey for the loan of these plates. They have each an area of 150 sq. cm. exposed to one another within the electrolyte, and are placed in a porcelain vessel $\frac{1}{8}$ in. apart. Pieces of varnished wood were placed at the back of each plate so as to prevent conduction between the outside surfaces through the fluid. The solution used was of water 100 parts by volume, and H_2SO_4 5 parts. Fig. 1 gives a diagram of connections, in which A, B are the terminals of a Siemens

FIG. 1.



W12 alternator, C, C are the cells above described, in series with which is placed a non-inductive resistance, DE. By means of a two-way switch, F, one of Lord Kelvin's quadrant electrometers, Q, could be placed across the cells C, C or the non-inductive resistance DE through a revolving contact-maker,* K, fixed to the shaft of the alternator. A condenser of about 1 m.f. capacity was placed across the terminals of the electrometer.

* For description of contact-maker see 'Roy. Soc. Proc.,' vol. 53, p. 357.

From observations of the values of the E.M.F. across the cells C, C at different times in a period, a Curve A (figs. 2, 3, 4) was plotted, giving potential in terms of time.

Fig. 2.

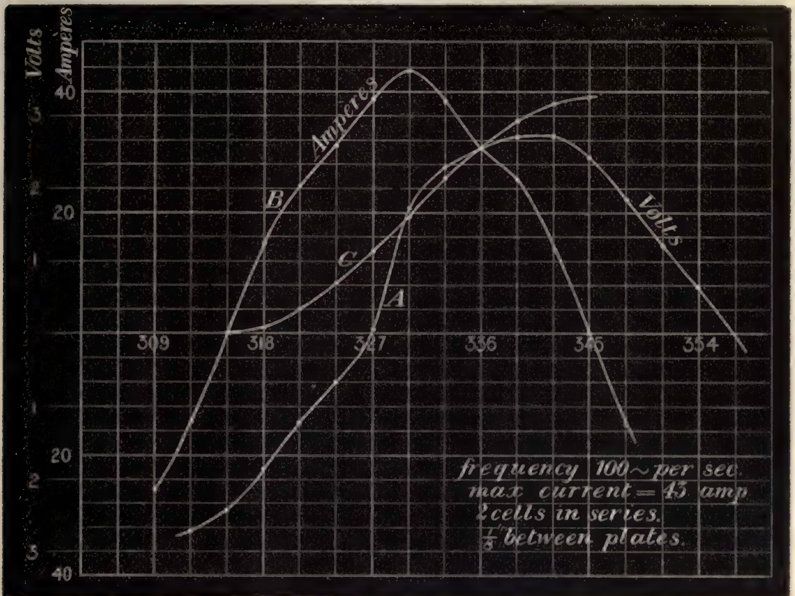


Fig. 3.

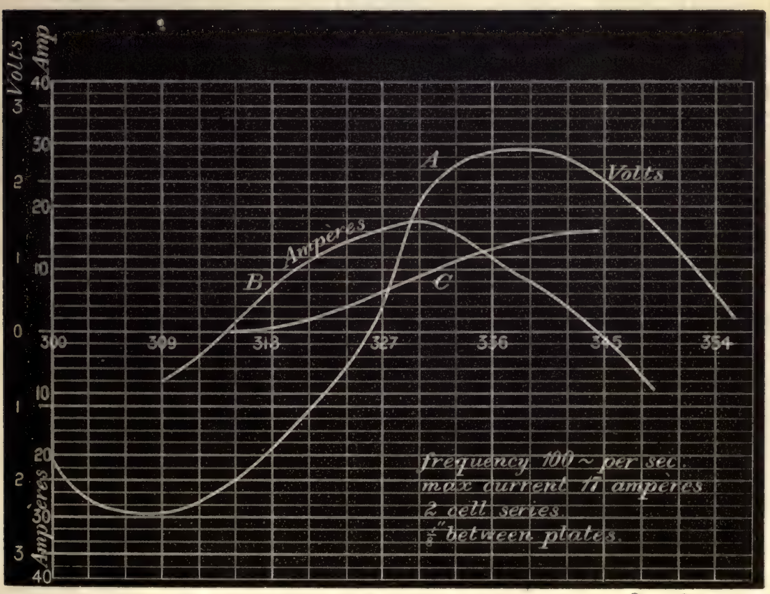
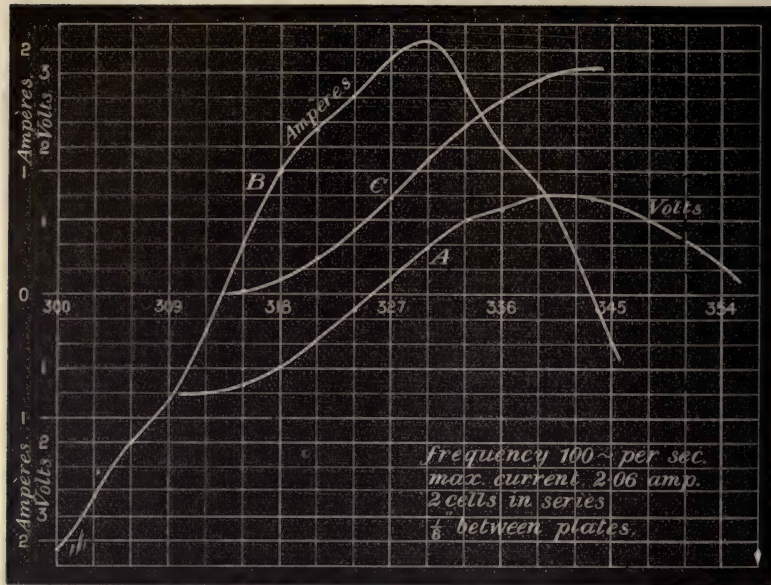


FIG. 4.



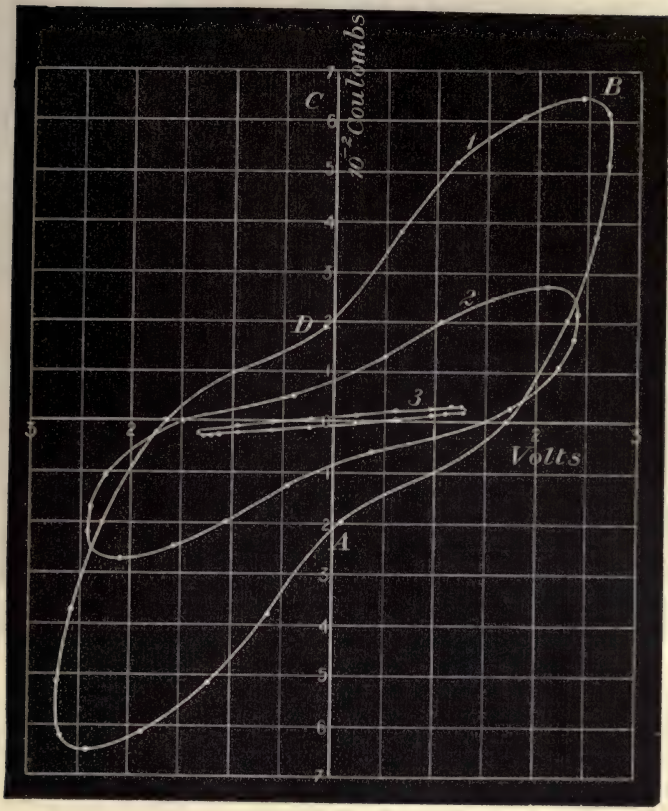
In the same way the Curve B was plotted for the E.M.F. between D and E, giving the current in terms of time. Hence the area of this Curve B up to any point, *plus* a constant, is proportional to the quantity of electricity corresponding to that point. This is shown in Curve C, which is the integral of B. The three curves, Nos. 1, 2, 3, in fig. 5, have been plotted from figs. 2, 3, 4 respectively, and show the cyclic variation of the potential across the cells in volts, and the

Table I.

	Frequency.	Maximum volts across cells.	Maximum ampères.	Maximum coulombs.	Area of cyclic curve in square centimetres.*	Efficiency per cent.
Fig. 2 ..	100	2·7	43·3	0·065	53·8	23
„ 3 ..	„	2·4	17·4	0·027	9·0	24
	„	2·38	10·0	0·0164	—	34
	„	1·93	5·7	0·0088	—	—
	„	1·61	2·9	0·0048	—	32
„ 4 ..	„	1·3	2·06	0·003	0·6	43

* 1 sq. cm. represents $\frac{1}{2}$ volt $\times 10^{-2}$ coulombs.

FIG. 5.



quantity of electricity in coulombs. The area of each curve (see Table I) is a measure of the energy dissipated per cycle, and since in this case there can be no accumulation of recoverable energy at the end of the cycle, it follows that the *whole* difference between what is spent during one part of the process and what is recovered during the other part is dissipated. In order to obtain an idea of the efficiency to be looked for when used as a condenser with platinum plates $\frac{1}{8}$ in. apart and dilute sulphuric acid, under varying conditions as to maximum coulombs, the area ABC (Curve 1, fig. 5) has been taken and is a measure of the total energy spent upon the cell; whilst the area DBC is a measure of the energy recovered—the ratio of these areas gives the efficiency.

Part II.—In the next set of experiments the frequency was varied, in addition to current; and in order to allocate the losses of potential in the cell, the platinum plates were placed $\frac{1}{4}$ in. apart for the

purpose of introducing an electrode into the fluid between the plates. This electrode consists of a platinum wire sealed into a glass tube which was capable of being placed in any desired position between the plates. The solution was, as before, of water 100 parts and H_2SO_4 5 parts by volume.

The arrangement of connections was similar to that shown in fig. 1, but, instead of observing the potential between the two platinum plates, observations were taken of the values of E.M.F. between one plate and the exploring electrode.

Table II gives particulars of the experiments tried, and two sets of results are shown in figs. 6 and 7, in each of which, from observations of the values of E.M.F. between the exploring electrode and the platinum plate at different times in a period, a Curve A_1 was plotted, giving potential in terms of time. This Curve A_1 is peculiar, in that the ordinates at corresponding points in the two half periods are not equal to one another, as is the case in Curve A, which gives the potentials across the two plates.

Table II.

	Frequency.	Maximum coulombs.	Maximum ampères.	Maximum volts per cell.
	100	0·090	58·6	1·83
	19·7	0·082	11·2	1·57
	20·5	0·054	7·1	1·39
Fig. 6......	142·5	0·071	65·4	1·77
„ 7......	2·4	0·120	1·9	1·37

The Curve A_1 gives, at any epoch, the potential taken up in the evolution of gas at the surface of the plate, *plus* the potential due to the current in overcoming the resistance of the electrolyte itself. To separate these quantities experiments were made upon the resistance of the electrolyte for varying frequencies and currents. To this end the plates were placed about 2 in. apart in the fluid, and two exploring electrodes, as already described, were placed within the fluid in a straight line drawn perpendicularly between the faces of the plates, the distance between the electrodes being 4·3 cm. Some difficulty was experienced, owing to the gases being given off at the plates more rapidly in some cases than in others. We, however, estimate that the resistance of a layer of the electrolyte, of a thickness equal to the distance between the electrode and plate, and of area equal to the area of plate submerged, in figs. 6 and 7, was approximately 0·0056 ohm.

In fig. 6 the Curve A_2 is the result of correcting Curve A_1 for

FIG. 6.

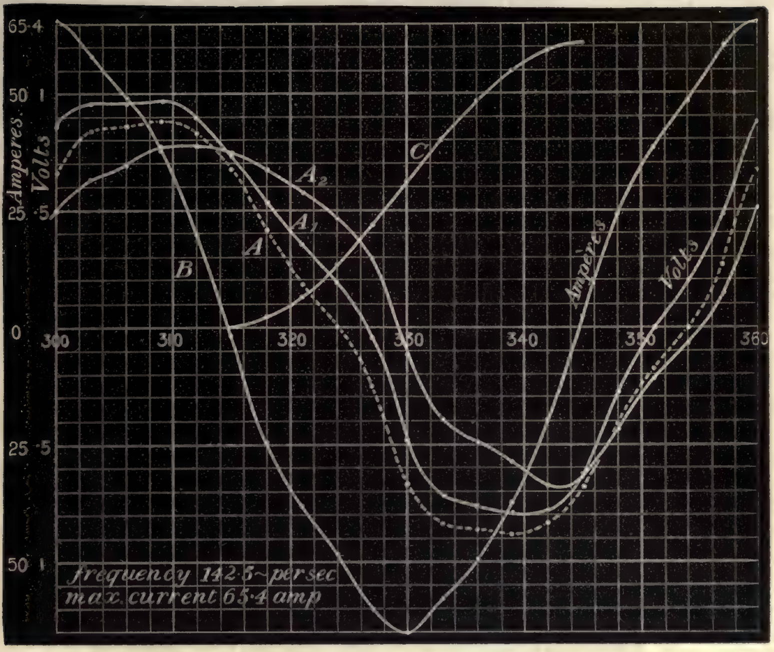
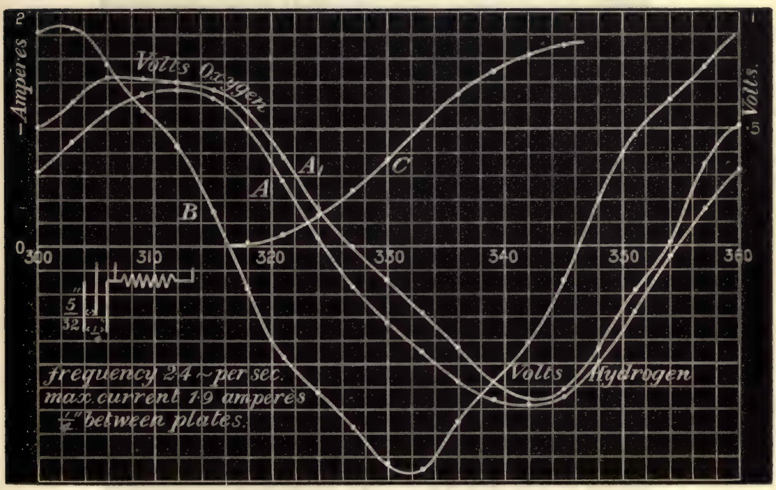


FIG. 7.

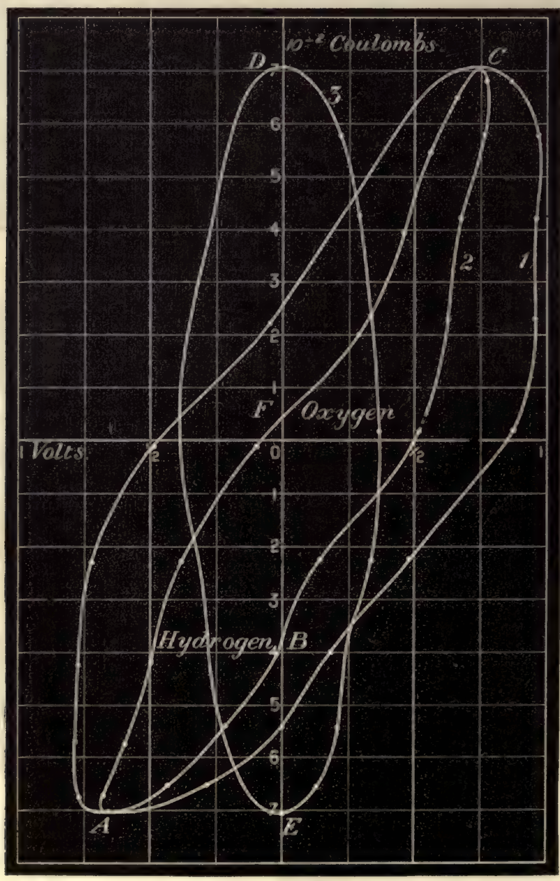


potential lost in the resistance of the electrolyte itself, and this curve therefore gives potential taken to decompose the fluid, in terms of

time. Curve B gives current in ampères in terms of time, whilst C is the integral of B and gives quantity in coulombs. With this frequency and current the energy dissipated on resistance of the electrolyte is a large proportion of the total energy dissipated; and only about 40 per cent. of the total energy is taken up in evolving oxygen and hydrogen at the plate, owing to the high frequency. The reverse of this is the case with lower frequency, as will be shown in connection with fig. 7.

From observations on the direction in which the electrometer needle was deflected for a given position of a Clark's cell connected to its terminals, we were able to state, for a given half period in the curves in figs. 6 and 7, which gas was being given off at the plate.

Fig. 8.



The abscissæ of Curves Nos. 1 and 2 (fig. 8) have been plotted from Curves A_1 and A_2 respectively in fig. 6, the ordinates being given for corresponding epochs by the integral Curve C.

Curve No. 1 (fig. 8) shows the cyclic variation of the potential between the electrode and the platinum plate, in terms of coulombs. Curve No. 2 shows the cyclic variation of the potential used in decomposition, also in terms of coulombs. Oxygen begins to be directed to the plate at the point A, as then the coulombs are a maximum and the current changes sign. But the oxygen is evolved on a hydrogen plate, and the E.M.F. aids the current; the work done on the plate is negative. This continues to point B (Curve No. 2). After this point (B) the character of the plate is that of a layer of oxygen and the work done becomes positive; this continues to the point C. The area AEB is the work returned by the plate, whilst oxygen is being evolved on a hydrogen surface. The area BCD is the work done on the plate, whilst oxygen is being evolved on an oxygen surface. In like manner the area CDF is the work returned by the plate whilst hydrogen is being evolved on an oxygen surface, and FAE the work done on the plate whilst hydrogen is being evolved on a hydrogen surface. The above areas have been taken in square centimetres, and are given in Table III. The area inclosed by Curve No. 2 (25·3 sq. cm.) represents the total energy dissipated by electrolytic hysteresis, whilst the area of Curve No. 1 (63·5 sq. cm.) gives the total energy spent in the cell. The abscissæ of Curve No. 3 are the differences of potential differences of Curves Nos. 1 and 2, the ordinates, as before, being coulombs. In fig. 8, 1 sq. cm. $= \frac{1}{5}$ volt $\times 10^{-2}$ coulomb.

In fig. 7 the frequency is 2·4 per second, and this is the case in which practically the whole of the energy dissipated in the cell is spent in decomposing the electrolyte at the plates. The correction to be applied to Curve A_1 for resistance is so small as to be almost negligible. The cyclic curve in fig. 9 has been plotted from Curve A_1 and the integral Curve C, and its area (146·7 sq. cm.) represents the energy dissipated per cycle by electrolytic hysteresis. Areas have been taken in square centimetres from the curve, as in the preceding case, and are given in Table III. In fig. 9, 1 sq. cm. $= \frac{1}{10}$ volt $\times 10^{-2}$ coulomb.

The potential curve in fig. 7 does not exhibit a level part at the highest potential; this is possibly due to the resistance of liberated gas.

A general conclusion of the experiments is that about one-tenth of a coulomb suffices to fully polarise 150 sq. cm. of platinum. This will liberate 0·00001 of a gram of hydrogen; hence 0·00000007 gram of hydrogen serves to polarise 1 sq. cm. of platinum. 0·00000007 cm. is probably a magnitude comparable with the distance between mole-

FIG. 9.

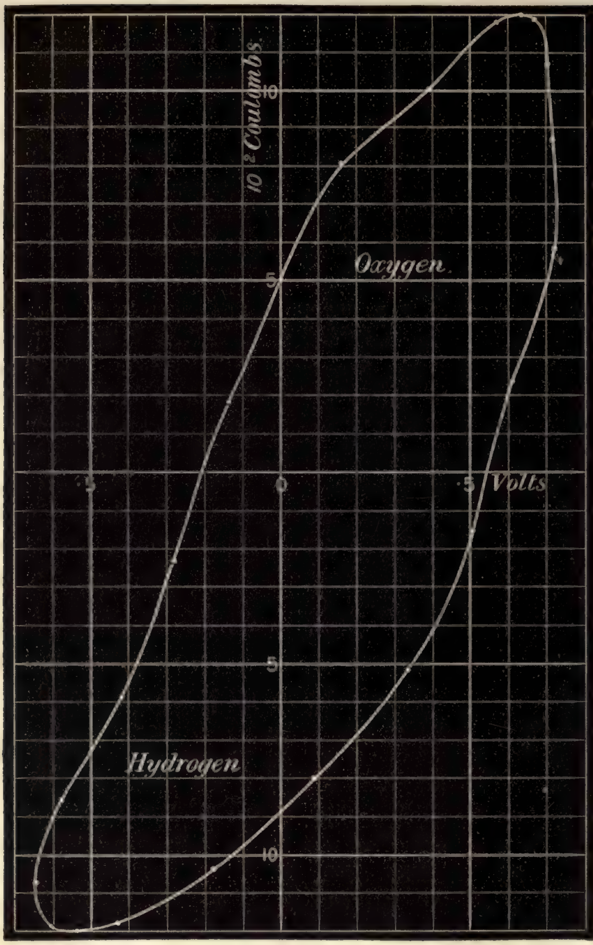


Table III.

	Oxygen on hydrogen surface, AEB.	Oxygen on oxygen surface, BCD.	Hydrogen on oxygen surface, FCD.	Hydrogen on hydrogen surface, FAE.
Fig. 8, Curve No. 2	3·65	27·25	13·8	15·5
Fig. 9	5·8	111·3	17·2	58·4

cules of hydrogen when this body is compressed to a density comparable with the density of liquids.*

"The Experimental Proof that the Colours of certain Lepidopterous Larvæ are largely due to modified Plant Pigments derived from Food." By EDWARD B. POULTON, M.A., F.R.S. Received May 12,—Read June 8, 1893.

[PLATES 3 AND 4.]

In a paper printed in the 'Proceedings of the Royal Society' for 1885 (pp. 269—315), I brought forward many reasons for regarding certain elements of the colouring of Lepidopterous larvæ as modified chlorophyll derived from the food plant. For this altered pigment the name metachlorophyll was suggested (*loc. cit.*, p. 270). Many other observations, subsequently made, supported the same conclusion; but it was not until the summer of last year (1892) that I was able successfully to carry out the critical experiment, viz., selecting a species of larva which normally eats green leaves, to feed it from the egg upon parts of the plant from which all colouring matter is absent.

This experiment was carried out in the following manner:—

A captured female of *Tryphœna pronuba* laid many hundreds of eggs in a chip box. The first larvæ began to appear September 7, 1893. On this and the subsequent dates, the larvæ intended for the purposes of these experiments were arranged in three sets, fed respectively upon—(1) the yellow etiolated leaves from the central part of the heart of the cabbage, (2) the white mid-ribs of such leaves from which the yellow blade was carefully removed with scissors, (3) the deep green external leaves of the same plant.

In all other essential respects the conditions of the three sets were the same. All were kept in the dark to prevent the change of the etiolin into chlorophyll. They were only exposed to light during the times necessary for comparison and feeding, and these are indicated below. A few were kept in glass cylinders standing on plates, the majority being confined in white earthenware pots covered at first with white muslin, but subsequently with glass sheets. Eventually all were kept in pots.

It is clear that the only essential difference between the conditions of the sets was the fact that the food of the first contained etiolin but

* Lord Kelvin states that in "any ordinary liquid" the mean distance between the centres of contiguous molecules is, with a "very high degree of probability," less than 0·0000002 and greater than 0·000000001 of a centimetre. See 'Roy. Institution Proc.,' vol. 10, p. 185.

no chlorophyll, while the food of the second contained only a little etiolin, and that so situated (around the fibrovascular bundles and buried deeply in the substance of the mid-ribs) that the larvæ could not make use of it, while the food of the third contained abundant chlorophyll.

It is evident that any constant difference between the larval colours in the three sets followed from these differences in the food supplied them; and, unless there are reasons for believing that the differences were due to pathological change, and thus an *indirect* result of the food, we must hold that they are a *direct* result, and that important elements of the larval colouring are dependent on the existence of some modification of pigments derived from their food.

The course of the experiment will now be described in a tabular form.

Dates.	(1) Etiolated leaves.	(2) White mid-ribs.	(3) Green leaves.
Sept. 7	18 larvæ, the first to hatch, introduced.		
Sept. 8	45 larvæ added.	About 50 larvæ introduced into 2 pots.	75 eggs, from which the larvæ were hatching, introduced.
Sept. 22	Re-fed; 40 placed in a cylinder and about 18 in a pot.	Re-fed; about 20 alive in each pot.	Re-fed; about 30 larvæ counted.
Sept. 27	Length about 8·5 mm. Some of these larvæ were distinctly, although rather faintly, green. 20 removed from cylinder into new pot.	All white, with no greenish tinge. 10 removed and placed in a bottle, and 10 in a new pot.	Re-fed; 28 or 29 counted.
Oct. 4	The longest larva 15·75 mm. long when extended in walking. Many changing skin. The black subdorsal semilunar marks distinct in many of the largest. Most were pale, but distinctly green, and all apparently with *some* green shade; very different from the pale yellow colour of the etiolated leaves. The larvæ remaining in cylinder were now placed in a pot.	Again re-arranged: 9 placed in one pot, 9 in another, and 11 of the smallest in a third.	28 counted; 1 killed accidentally; 13 placed in one pot, and 14 in another.
Oct. 10	All very carefully compared. 27 in 4th stage, very uniformly about 23·0 mm. long, when fairly extended at rest; 5 *pale distinct green;* 12 *dark greenish,* varying according to the amount of dark superficial pigment, and transitional into	24 alive. Again re-arranged in 4 groups, placed in separate pots, containing respectively the 5 largest, the 9 next in size, the 5 next in size, and the 5 smallest. The largest were in 2nd stage. All quite white, being quite as large as those fed	These larvæ were frequently compared together and with those of experiment (1). There was no essential difference, except that these grew rather more slowly. At this time they were similarly

Dates.	(1) Etiolated leaves.	(2) White mid-ribs.	(3) Green leaves.
	the *brown* larvæ (10) in which the green colour is absent. The palest green larva, a similar larva with more superficial pigment, a typical dark greenish, and a typical brown larva were selected for painting at this date, together with a *very* deep bluish-green larva, which changed its 3rd skin just before being painted (see Pl. 3, fig. 1). The shade of green was far deeper than in any other individual. 11 larvæ in 3rd stage, the largest being 18—19 mm. long when extended. 7 *pale green*, 1 *pale brown*, 3 *deep brown.* 23 larvæ changing 3rd skin. 13 *pale green*, 5 *darkish green*, including the deep bluish-green one represented in fig. 1, Pl. 3 (4 of these changed their skins during the comparison), 6 *brown* (2 being greenish and transitional).	upon etiolin (1) on Sept. 27, when they began to become green.	green and brown of various shades, darkening, as they entered the last stage, into brown.
Oct. 11		3 of the largest were figured (see Pl. 3, fig. 2). They were in the 2nd stage.	
Oct. 19	The larvæ had grown rapidly, and a few had entered the last stage, while several were changing the last skin. As they matured they became darker, and all	5 largest were all about 11 mm. long when extended; 3 were changing 2nd skin, 2 large in 2nd stage. Same white appearance with faint greyish shade, due to	

Dates.	(1) Etiolated leaves.	(2) White mid-ribs.	(3) Green leaves.
	eventually turned brown, mostly of a dark shade.	superficial true pigment. Of the 9 next in size 3 were dead, and the 5 smallest were all dead or dying.	
Oct. 26	Nearly all in last stage, and brown of various shades, occasionally faintly greenish.	The 2 largest were 12·0 and 13·5 mm. long respectively when extended. Of the 6 next in size 2 were dead, and of the remaining 5, 2 were dying.	
Oct. 29		Of the 5 largest, 4 were dead by this date or in the course of the next day or two. The remaining larva was placed (Oct. 29) on etiolated leaves.	
Nov. 2	Mostly mature, and various shades of brown. When carefully compared with the larvæ fed on green leaves (3) no difference in colour could be seen, except such as was due to the colour of the food in the digestive tract. This was also true of earlier stages.	The larva on etiolin remained as white as before. Of the next in size 3 were alive, 1 of which had grown very much, and seemed to be near the end of the 3rd stage. It was very white and maggot-like. The smallest was placed on etiolin. Of the 5 remaining larvæ 2 were alive. 1 was very small, and apparently in the 1st stage, the other probably at end of 2nd stage. Both were placed on etiolin.	Carefully compared. 23 alive in 4 pots. 20 in last stage, and all various shades of brown except 2, which were slightly greenish and distinctly greenish respectively, the latter having only just changed skin. 1 was in 4th stage; 2 were changing last skin (1 green and 1 light brown).
Nov. 9		Of the only 2 larvæ now eating the white	The 3 last mentioned larvæ and the

Dates.	(1) Etiolated leaves.	(2) White mid-ribs.	(3) Green leaves.
		mid-ribs the larger one had grown considerably. The 4 on etiolin were still alive, and seemed to have become yellower, but not more so than was to be expected from the food in the digestive tract.	distinctly greenish one in last stage were kept together and compared at this date. 1 was dead; 1 was small in last stage (dark brown); 2 were large in last stage (1 light brown and 1 somewhat greenish-brown).
Nov. 16		Of the 4 on etiolin 2 were dead (including the larva first placed on it), the others still faintly yellowish.	
Nov. 20		The largest larva was now 21·5 mm. long when extended at rest; the other was dead. Only 1 larva alive on etiolin, and that died Nov. 21.	
Nov. 28		The remaining larva had grown greatly, and was advanced in the 4th stage. A great development of superficial pigment had appeared suddenly between this date and Nov. 20, especially marked upon the brown head, prothoracic plate, and supra-anal plate, and in the dark subdorsal semilunar marks; minute black points were also abundantly scattered over the whitish general surface. The white sub-	

Dates.	(1) Etiolated leaves.	(2) White mid-ribs.	(3) Green leaves.
		dorsal line was opaque, and evidently followed from some structural cause (probably pigmentary) distinct from that to which the pale whitish general surface was due. This was perhaps the case with the white spiracular line also. When at rest the length varied from 20·0 to 22·0 mm. according as the larva was moderately contracted or moderately extended. On December 2 the larva was painted (in the 4th stage) in two attitudes (see Pl. 4, fig. 1).	
Dec. 16		The larva was resting before changing the last skin; it was 29·0 mm. long when extended at rest.	
Dec. 18		The last skin had now been shed, and the larva was *much* darker, although the ground colour seen between the dark pigment spots was as pale as before. Hence the effect was greyish. On the evening of the 17th, before ecdysis, the increase of superficial pigment was observed, especially in the dorsal, subdorsal, and supra-spiracular regions.	
Dec. 29		The larva was painted when much grown in last stage (Pl. 4, figs. 2 and 3).	

Dates.	(1) Etiolated leaves.	(2) White mid-ribs.	(3) Green leaves.
Jan. 1, 1893		At this date the larva was offered etiolated leaves, but did not appear to eat at all, and seemed to be hybernating. The appearance did not alter. On Jan. 14th it was killed accidentally.	

Before discussing the conclusions, it will be best to consider the possible effect of certain conditions incidental to the experiments.

Darkness.—The almost continuous darkness probably affected the colours of the larvæ in (1) and (3), as they became mature. The adult larvæ of *T. pronuba* are sometimes bright green; and some of the larvæ, hatched from the batch of eggs which supplied these experiments, reared by Miss L. J. Gould in the light and with green surroundings, remained a distinct, although dark, green until maturity, quite unlike any of those referred to above.

Although it is thus probable that the larvæ are sensitive, like so many others, to the colour and degree of illumination of their surroundings, the results of these experiments are not affected in any essential respect; for there was abundant opportunity for comparison *before* the changes referred to had taken place (*e.g.*, October 10), and when the majority of the larvæ in (1) and (3) were green (see Plate 3, fig. 1). Furthermore it is also evident that the comparison was equally valid *after* the change had taken place, inasmuch as the brown ground colour, no less than the green, is shown to be due to a modified plant pigment.

Nutritive Value of Pigmentless Food.—The extremely slow growth of the larvæ fed upon the white mid-ribs, and the death of all except one of them in an early stage, may be looked upon as an argument that they were in a pathological condition, one result being the inability to form a certain kind of pigment. Such an interpretation would, of course, upset the conclusions I have arrived at.

On the other hand, it may be urged that the single larva in (2) which survived until it was advanced in the last stage was certainly not pathological, and yet was unable to form the pigments in question. Although it grew very slowly in early youth, it began to be conspicuous by its size on November 2, and from this date it grew rapidly and fed largely (see Plate 4, figs. 1 and 2); judged by all standards, it was perfectly healthy. Furthermore, everyone who breeds larvæ knows that they are subject to diseases of various kinds, and yet, so far as I am aware, the complete inability to form certain classes of pigment has not been recognised as a symptom. Moreover, the larvæ fed on the thick succulent etiolated leaves (1) grew far more rapidly then those fed on green leaves (3). This species hybernates in the larval state, and, as is usually the case in such species, the rate of growth is extremely irregular. Many of the larvæ reared by Miss Gould, and single larvæ fed upon green leaves by me, lagged far behind the others and yet remained healthy.

I believe that the retarding effect of the mid-ribs was not due to the absence of plant pigments, but to the rapid drying and oxidation of the cut surfaces (left by the removal of the rest of the leaf) and the inability of the young larvæ to get sufficient food from other parts,

where the tough cuticle could not be easily penetrated by their small weak mandibles. The larger larvæ do not experience the same difficulty.

It will be well, however, to repeat the experiment with other larvæ, some of which may be expected to have greater powers of endurance. I would suggest *Mamestra brassicæ* and *Phlogophora meticulosa* as suitable for the purpose. Freshly cut mid-ribs might be offered every day or perhaps twice a day.

Conclusions from the Experiments.—Assuming that the results obtained in Experiment (2) are not pathological, and I believe that this assumption is justified, it follows that etiolin (1), no less than chlorophyll (3), can be transformed into a larval colouring matter, which may be either green or brown, and is so disposed as to form a ground colour.

The fact that *brown* pigments may be thus formed is new. In my previous paper ('Roy. Soc. Proc.,' 1885, pp. 269 *et seq.*) I gave reasons for the conclusion that the green pigments are derived from plants, but argued that brown pigments are proper to the larva. This still remains true in many cases. Thus the green larvæ of *Amphidasis betularia*, investigated in 1892, are coloured by derived pigments contained in the superficial fat, while the brown larvæ are coloured by true pigment contained in the epidermic cells ('Trans. Ent. Soc. Lond.,' 1892, pp. 357—359), so that the green fat which lies beneath is concealed. The intensely opaque and dark larvæ of many other Geometræ are probably similarly coloured by true pigments in the cuticle or epidermis. But the brown ground colour of many Noctua larvæ will probably be found to be due, like that of *T. pronuba*, to modified plant pigments.

A comparison of the larvæ fed on pigmentless food (Plate 3, fig. 2, Plate 4, figs. 1—3) with those fed upon etiolated leaves (Plate 3, fig. 1) and the similar larvæ fed upon green leaves, proves that both green and brown ground colours are modified plant pigments. When the larvæ fed on etiolin were being compared on October 10, one of them became irritated and expelled a drop of fluid from its mouth. This fluid was of a faintly *bluish-green* colour. This observation suggests that the change of etiolin into a soluble green pigment takes place in the digestive tract. Chlorophyll similarly becomes soluble and forms a green solution (turning brown on exposure) in the digestive tract of larvæ. It is possible that the brown ground colour of the larvæ is also a result of oxidation: at any rate, it is a change in the direction of greater stability; for I have shown that the colours of certain brown larvæ, evidently coloured like those of *T. pronuba*, are far more persistent after preservation than those of the green varieties of the same species ('Roy. Soc. Proc.,' 1885, pp. 275, 276).

Although the brown ground colour, probably situated in this

species in the epidermic cells, is thus derived, there is an abundant deposit of true pigment in the form of spots and patches in the superficial cuticle. This was as distinct in the larvæ of (2) as in those fed upon etiolin or chlorophyll; but, the ground colour of the former being white instead of green or brown, it produced a greyish effect (Plate 4, figs. 1—3). The opaque, white stripes in the subdorsal and spiracular regions are also probably due to true pigment situated in this case in the epidermic cells, and are equally conspicuous in the larvæ fed on pigmentless food (Plate 4, fig. 2).

In certain parts of the body the cuticle is of relatively greater thickness—the head, prothoracic dorsal plate, supra-anal plate, true legs, and parts of the claspers. In these situations, therefore, the combination of a deeply-placed ground colour composed of derived pigments with a superficially placed true pigment would not necessarily produce the same effect as in the other parts of the body where the cuticle is much thinner; for the derived pigments would tend to be hidden. In these parts, therefore, *both ground colour and markings are cuticular, while both are composed of true pigment* of such a tint as to harmonise with the effect produced by the combination of two distinct elements in other parts of the body. Hence these parts remained normal in the larvæ of Experiment (2), resembling the brown larvæ of the other experiments, and serving to show what the colour of the rest of the body would have been if the plant pigments had been present in the food (Plate 4, fig. 3).*

Some indication was afforded in the course of these experiments that the power of converting the plant-pigments into metachlorophyll may be lost in larvæ which have been fed from the egg for a considerable time upon pigmentless food. Thus the larvæ of Experiment (2) remained pale when fed upon leaves which caused those of Experiment (1) to become brown or green. At the same time it must be remembered that these particular larvæ were certainly unhealthy, and died soon after the change of food. I hope to repeat this experiment upon healthy larvæ. I have already shown that many larvæ which are normally found upon a variety of food plants will starve rather than eat certain of them when they have been fed upon the others from the egg ('Ent. Soc. Lond. Trans.,' 1887, pp. 312—314). It is possible that a somewhat analogous "gastric education" may take place as regards the digestive action upon plant pigments. But confirmatory experiments, specially directed to test the conclusion, are much wanted.

* [This argument appears to be valid in the case of the older larvæ of this species and probably many others. There are, however, many instances in which the derived pigments are distinctly visible through an extremely thick cuticle (*e.g.*, in the head of larvæ of the genus *Smerinthus*). The distribution of the derived pigments has not been investigated in this case.—October 15, 1893.]

It is of great interest that the etiolin should be as effective as chlorophyll in the production of larval colours. It is, however, probable that the difference between etiolin and chlorophyll is, chemically, extremely small, while both appear to undergo similar changes in the larval digestive tract, yielding a substance which becomes dark coloured on exposure to air, probably by oxidation. Thus of the two heaps of fæces represented in Plate 3, fig. 1, that to the left had been exposed to the air for some hours, and was dark brown, while that to the right was fresh and of a pale-yellow tint. A cut midrib darkens on exposure quite independently of the plant pigments as may be seen in the same figure; but the tint is different, and the depth of shade far less than in the fæces containing abundant etiolin. The fæces of the larvæ fed on pigmentless food similarly darkened far less rapidly and to a much less extent than those of the others.

Although the results of these experiments are, I believe, completely successful in establishing the conclusion they were intended to test, it must be admitted that they point to the beginning of an investigation rather than its end. We now know that certain larval colours are dependent on the existence of modified plant pigments, and this naturally leads to an enquiry into the nature and causes of the processes by which chlorophyll and etiolin are converted in the animal body into a comparatively stable green or brown substance far removed from its original position in the digestive tract, and situated so as to form an important element in the effective colouring of the individual.

Description of Plates 3 and 4.

Plate 3.

Fig. 1.—A group of five larvæ of *Tryphœna pronuba* in the 4th stage, natural size. These larvæ had been fed entirely upon the etiolated leaves of cabbage. They had hatched September 7 and 8 (1893) and were painted October 10. Nearly all the shades of colour observed in the larvæ at this stage are represented in the figure, four being various shades of green, and one brown. It is clear from the figure that the larvæ can form a deep green colouring matter from etiolated leaves. There was, in fact, no difference in this respect between them and larvæ fed on green leaves.

The marked contrast in colour between the green larvæ and the leaf is some indication of the change which the etiolin has undergone in the larval body. The dark marks along the sides are due to superficially placed true pigment, which is formed independently of any coloured substance in the food plant.

Two heaps of fæces are represented in the figure: that to the right

1.

Natural Size

2.

×2

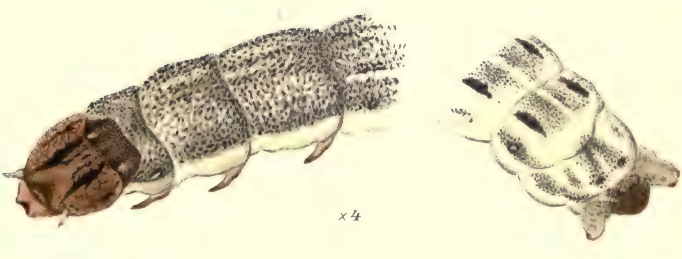
1.

Natural Size

2.

Natural Size

3.



×4

P. J. Bayzand pinx. Lith. Werner & Winter, Frankfort°M.

fresh, and pale yellow in colour; that to the left exposed to the air for some hours, and dark brown.

Fig. 2.—A group of three larvæ, of the same species, in the 2nd stage, twice the natural size. These larvæ had been fed entirely upon the white mid-ribs of cabbage leaves. They had hatched September 8, and were painted October 11. While the larvæ represented in fig. 1 were rather larger than those fed on green leaves, these are *much* smaller. The colour is white, and maggot-like, the faint greyish appearance being due to superficial true pigment. Except upon the head, there is not a trace of either the green or the brown ground colour invariably found in the larvæ of this species under normal conditions. Furthermore, these larvæ are uniform in appearance, although the normal larvæ are extremely variable.

A comparison between figs. 1 and 2 proves that the brown or green ground colour of the species is due to some modification of etiolin (or chlorophyll in the case of normal larvæ), unless indeed the results are to be explained as pathological—an interpretation opposed to the facts represented in the figures on Plate 4.

Plate 4.

Fig. 1.—Out of about fifty larvæ which hatched September 8, and were fed on white mid-ribs, a single one began to be conspicuous by its size on November 2, and from this time it grew rapidly, and was evidently quite healthy, although all the others were dead by November 21. On December 2 it was painted (natural size) in two positions, being in the 4th stage. The ground colour remained white or cream-coloured; the grey effect being caused by superficial true pigment, which is seen to be especially marked upon the brown head, claspers, thoracic legs, prothoracic, and supra-anal plates, and upon the dark subdorsal semilunar marks.

Fig. 2.—The last skin was changed by December 18, and, on the 29th, the larva was again painted of the natural size, when advanced in the last stage. The ground colour remained the same, but an increase in the true pigment caused the larva to become a darker shade of grey. A row of supra-spiracular dark markings also made their appearance. The white subdorsal line and, perhaps, the spiracular line were evidently due to some cause of colour (probably pigmentary) distinct from that on which the pale ground colour of the general body surface depended.

Fig. 3.—At the same date the head and anterior segments were painted, × 4 diameters. In those parts where the cuticle is thickened, the head, prothoracic plate, and thoracic legs, a brown ground colour (as well as the black spots and patches) is developed from true pigment in the cuticle itself. Hence these parts remain normal when

the larvæ are fed upon leaves without chlorophyll or etiolin. But over the general surface of the body the cuticle is very thin, and only contains the black spots and patches of true pigment, while the brown or green ground colour, derived from plant pigments, is subcuticular in position. Hence in a larva fed on pigmentless parts of leaves, represented in fig. 3, this latter ground colour is replaced by a creamy tint which is due to the uncoloured tissues of the body, especially the fat, and to the contents of the digestive tract. This creamy ground colour, combined with the spots of true pigment in the cuticle, produces the general greyish appearance of these larvæ.

The posterior segments of the larva, painted at the same date, × 4 diameters, are also represented in the figure, indicating that the thickened cuticle of the supra-anal plate (which possessed a sharper outline than that represented in the figure) contains both brown ground colour and black spots of true pigment, while the general surface presents the combination of a white ground colour and dark spots, conferring a grey appearance.

December 7, 1893.

The LORD KELVIN, D.C.L., LL.D., President, in the Chair.

A List of the Presents received was laid on the table, and thanks ordered for them.

The President announced that he had appointed as Vice-Presidents—

The Treasurer.
Sir John Lubbock.
Dr. Perkin.
The Marquis of Salisbury.

The following Papers were read:—

I. "The Organogeny of *Asterina gibbosa*." By E. W. MACBRIDE, B.A., Demonstrator of Animal Morphology to the University of Cambridge. Communicated by ADAM SEDGWICK, F.R.S. Received September 13, 1893.

Having been engaged in studying the development of *Asterina gibbosa* during the past year, I think it advisable to publish an account of the results already obtained, as these are of considerable interest, and it may be long before I can complete my investigation. As this is only a preliminary account, I shall ignore statements which conflict with my results, noticing only the results of other workers in so far as they confirm my work.

The classic research of Ludwig* on this subject is well known. The advances I have been able to make on his work concern chiefly the later larval stages and the metamorphosis.

Ludwig finds a regular segmentation, giving rise to a ciliated blastula; typical embolic invagination follows, and the blastopore, placed at first in the centre of the ventral surface, is carried by growth to near the posterior end, and functions for a short time as larval anus. In the meantime the archenteron becomes divided into an anterior thin-walled portion, and a posterior thick-walled one. The latter becomes separated as the definitive gut, which is soon joined by the larval œsophagus, which arises as an ectodermic invagination, the former becomes the cœlom, sending back a horn on each side of the gut. On the left side, in the præoral portion of the cœlom, we find a pore opening to the exterior—the madreporic pore.

* "Entwicklungsgeschichte der Asterina gibbosa." 'Zeitschrift für Wiss. Zoologie,' vol. 37.

My results diverge from those of Ludwig at this point. What follows is the result of my own work. The *cœlom becomes segmented, as shown in fig.* 1, *and the arrangement of its divisions strongly recalls that*

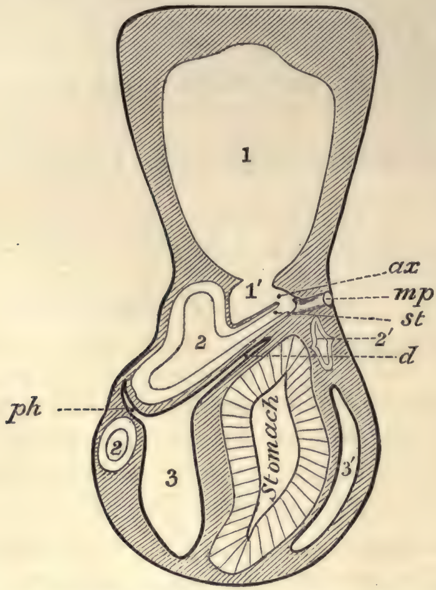


FIG. 1.—Diagram of a Longitudinal Horizontal Section through a late Larva of *Asterina gibbosa.*

of Balanoglossus. By the outgrowth of a pair of vertical transverse septa, a pair of posterior cœlomic cavities is separated off (3′ 3′, fig. 1), this separation being for a time incomplete ventrally. Simultaneously, two outgrowths from the anterior cavity appear, growing backward, and overlapping to some extent the posterior cavities. Of these, the left opens for a long while by a wide aperture into the anterior cavity, whilst the right is almost immediately completely segmented off.

These are the paired rudiments of the water-vascular system, the *right* and the *left hydrocœle. In one larva I found them equally developed*, but usually the right hydrocœle has the form of a small closed vesicle, which persists for life in the neighbourhood of the madreporite. These cavities I compare to the collar cavities of *Balanoglossus*, and find support for this view in the structure of *Cephalodiscus*, where the collar cavities are prolonged into *long, pinnately-branched arms*, comparable to the radial canals of the water-vascular system of Echinoderms, with their rows of tube feet.

There is no hæmocœle in Asterina gibbosa; *all cavities lined with*

epithelium are derived from the cœlom. The radial perihæmal canals and their connecting outer circular perihæmal canal are derived from interradial diverticula of the cœlom, one of which (*Ph.*, fig. 1) is shown in the figure. The longitudinal septa in the radial canals are due to the apposed walls of adjacent interradial rudiments. The axial sinus (*ax.*, fig. 1) surrounding the stone canal is the posterior division of the anterior body cavity into which the pore canal opens. Bury* states that in a future paper he will prove this, but he has not fulfilled his promise. The stone canal is a groove in the neck of communication between the anterior cœlomic cavity; it becomes constricted off, and forms a tube opening at its distal end into the axial sinus, close to the inner end of the madreporic pore canal. This arrangement has been seen by Ludwig, but what he has not seen is that it persists in the adult, and hence he failed to recognise the rudiment of the axial sinus.

The dorsal organ, the heart or "central-blutgeflecht," is nothing more than an ingrowth of the left posterior cœlom into the septum separating the posterior cœlomic cavities from the axial sinus (*d*, fig. 1). It soon becomes solid. From its upper end in the adult the genital rachis grows out, as Cuénot† inferred, and I have elsewhere proved.‡

Since the genital organs are formed later, as local swellings of this rachis, the ultimate origin of the sexual cells, in *Asterina* as in Vertebrata and Annelida, is cœlomic epithelium. The aboral sinus surrounding the genital rachis is formed from a special diverticulum of the cœlom.

As Ludwig has pointed out, the præoral part of the larva becomes converted into a special locomotor organ. It is foot-shaped, has long cilia, and also functions like a tube-foot, as a temporary fixing organ. Ludwig did not observe, however, that during the metamorphoses, having given up its locomotor functions, is *converted into a permanent fixing organ or stalk.* This arrangement persists for some time after the larva has acquired the adult form, *for it is for a time unable to use its tube feet, and when displaced from its attachment floats helplessly about.* Bury§ has shown that the stalk of Crinoids is likewise the præoral lobe, and the free swimming larva of *Antedon* strongly resembles that of *Asterina*, the main difference being that the larval mouth, which soon closes in *Asterina*, is never formed in *Antedon*, in which also the anterior body cavity is of less extent,

* "Studies in the Embryology of Echinoderms," H. Bury, 'Quart. J. Mic. Sci.,' 1889.

† "Contributions à l'Étude Anatomique des Asterides," L. Cuénot. 'Archives de Zoologie Expérimentale,' T. v. *bis.*

‡ "The Development of the Dorsal Organ, Genital Rachis, and Aboral Sinus of *Asterina gibbosa*," E. W. MacBride, 'Zool. Anzeiger,' No. 419.

§ "The Early Stages of *Antedon rosacea*," H. Bury, 'Phil. Trans.,' 1888, B.

and is completely separated from the hydrocœle. The just fixed larva in both cases we take to represent the ancestor of Echinoderms, just after it had given up its free-swimming life (fig. 2). The curious, and

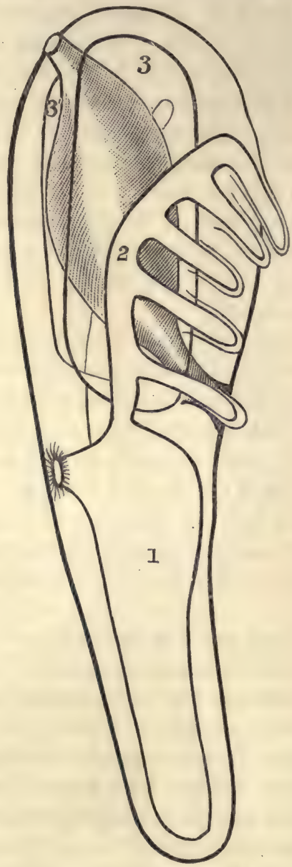


FIG. 2.—Supposed Ancestor of Asterids and Crinoids.

as yet unexplained, peculiarity of Echinoderms, the predominance of the left side (left hydrocœle and left posterior body cavity), soon made itself felt. Starting from this point, however, ontogeny plainly teaches us that *Asterina* and *Antedon* have diverged in two opposite directions. In *Antedon* an excessive growth of the ventral surface has rotated mouth and hydrocœle backwards and upwards away from the stalk (fig. 3). A precisely similar change to this takes place, as we know, in *Ciona* and *Pedicellina*, and it is to place the mouth in a favourable position to catch pelagic prey. In *Asterina*, on the other hand, the body is flexed ventrally on the stalk (fig. 4), so

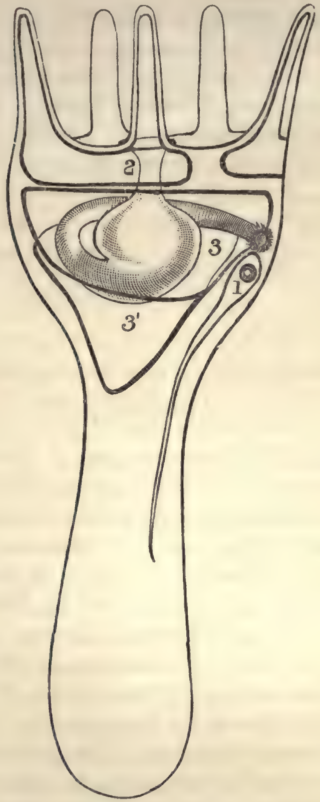


FIG. 3.—Early Stage in Development of Crinoids.

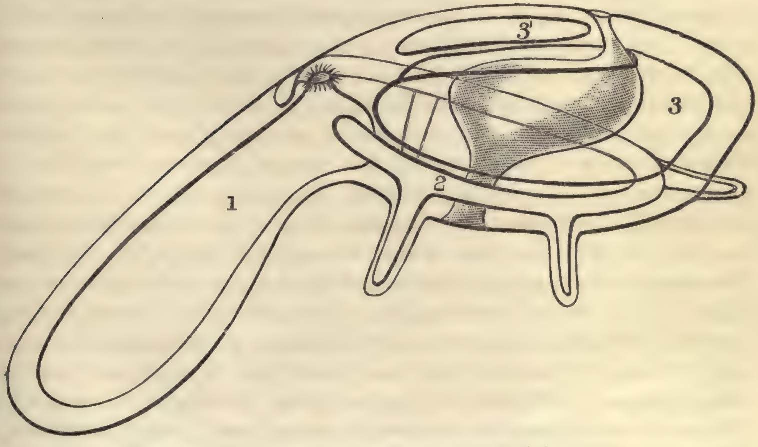


FIG. 4.—Early Stage in the Development of Asterids.

that the ends of the hydrocœle meet round it, and the mouth is approximated to the substratum, so that the animal can feed on the mud beneath it, which is impregnated with organic matter.

It follows that *the abactinal poles of* Asterina *and* Comatula *are not comparable with each other*, and that all conclusions based on the supposed homology of the dorsocentral in Echinids and Asterids, and that in Crinoids, are incorrect.

II. "Reptiles from the Elgin Sandstone:—Description of Two New Genera." By E. T. Newton, F.R.S. Received November 2, 1893.

(Communicated by permission of the Director-General of the Geological Survey.)

(Abstract.)

Since the reading of the previous paper "On some New Reptiles from the Elgin Sandstone" ('Phil. Trans.,' B, 1893), the author has received several additional specimens from the same formation in the neighbourhood of Elgin, but not from the same locality, and representing other groups of Reptiles. Two of these specimens, being new and interesting forms, are described in detail. One of them is the property of Mr. James Grant, of Lossiemouth, and is contained in a small irregular cube of sandstone. The bones themselves having been dissolved out, as in the earlier described fossils, their forms have been reproduced by gutta-percha casts taken from the cavities left in the stone. This reptile was evidently a small Parasuchian Crocodile, allied to *Stagonolepis;* it is now represented by the skull, which is about 3 inches long, and the anterior half of the body, with the pectoral arch and both the fore limbs. The skull is depressed, has a pair of supratemporal fossæ and a pair of orbits completely surrounded by bone, and in front of the latter, on each side, a large prelachrymal fossa; the two nasal openings are small, and placed near the end of the muzzle. The palate is narrow and deeply grooved, with primitive posterior nares placed far forwards. The teeth vary in size, are slender, conical, and recurved, and restricted to quite the anterior part of the upper jaw. The vertebræ are slightly biconcave; the 9th has distinct double articulations for the ribs, but how far this character extended forward is uncertain. The scapulæ are long and slender, while the coracoids are short and wide. There is an interclavicle. The humeri have each a strong pectoral crest, and are Crocodilian in form; the radius and ulna are slender bones; the carpals are indistinct; five metacarpals are present on each side, but only a few of the phalanges are to be seen. Above the vertebræ there is a double row of small, pitted, and closely-set scutes. This small Parasuchian is named *Erpetosuchus Granti.*

The second specimen was obtained by the Rev. Dr. Gordon, of Elgin, from the quarry at Spynie, where the *Telerpeton Elginense* was found, and will be deposited in the British Museum. In this fossil the bones were present, and the skull, thanks to the manipulative skill of Mr. J. Hall, of the British Museum, is still preserved, but many of the other bones were too much crumbled to show their form, and the casting process was again resorted to. This specimen must have been entire when buried in the sandy matrix, but the neck and fore limbs are now wanting. The resemblance of this fossil to *Aëtosaurus* was noticed by Mr. A. Smith Woodward when it was exhibited at one of the Royal Society Soirées. The skull is about 4½ inches long, sharp anteriorly, and Bird-like when seen from above, but deep when seen from the side. There is on each side a laterally placed nasal aperture, a large prelachrymal fossa, a wide orbit, and an infratemporal fossa. The teeth are of different sizes, but all seem to have been lanceolate, recurved, compressed, and serrated anteriorly and posteriorly as in *Palæosaurus* and *Cladyodon.* The palate is deep, and a median pair of apertures near the post-palatine vacuities are believed to be primitive posterior nares, placed far back, somewhat as in *Belodon.* This skull closely resembles that of *Ceratosaurus.* The more anterior of the thirteen presacral vertebræ which are present have distinct capitular and tubercular articulations for the ribs, but these gradually unite, and in the hindermost there is but one process with, perhaps, two articular surfaces at the extremity. All the vertebræ are biconcave. The sacrum includes three vertebræ with large and expanded ribs. There are twenty-one caudal vertebræ present, which have long neural and hæmal spines. The ilium is Crocodilian in form, but not so high as in *Stagonolepis*, and thus approaches to the Dinosaurian type. The ischia are elongated bones, but the pubes are still longer and are directed forwards. The tibia and fibula must have been about the same length as the femur, which is nearly 4½ inches long, and Crocodilian in form. The astragalus is free, and has some resemblance to that of a Crocodile ; there are five metatarsals, and the number of the phalanges is—to the first digit 2, to the second 3, to the third 4, to the fourth 5, to the fifth 3 or more. Many oval scutes are seen scattered above the neural spines, the anterior and larger ones being ornamented with irregular radiating pittings and ridges.

This reptile seems to be intermediate between the Dinosaurians and Crocodilians. The skull and teeth are most like those of Dinosaurs ; the pelvis and limbs might belong to either Dinosaurs or Crocodiles ; while the free astragalus is certainly a Crocodilian character. Provisionally this reptile is referred to the Theropodous Dinosauria, and is named *Ornithosuchus Woodwardi.*

III. "A Dynamical Theory of the Electric and Luminiferous Medium." By JOSEPH LARMOR, F.R.S., Fellow of St. John's College, Cambridge. Received November 15, 1893.

(Abstract.)

Ever since the causes of natural phenomena began to attract attention, the interaction of the different classes of physical agencies has been taken to suggest that they are all manifestations in different ways of the energy of some fundamental medium; and the efforts of the more sanguine class of naturalists have always been in some measure directed towards the discovery of the properties of this medium. It is only at the end of the last century that the somewhat vague principle of the economy of action or effort in physical actions —which, like all other general principles in the scientific explanation of Nature, is ultimately traceable to a kind of metaphysical origin—has culminated in the hands of Lagrange in his magnificent mathematical generalisation of the dynamical laws of material systems. Before the date of this concise and all-embracing formulation of the laws of dynamics there was not available any engine of sufficient power and generality to allow of a thorough and exact exploration of the properties of an ultimate medium, of which the mechanism and mode of action are almost wholly concealed from view. The precise force of Lagrange's method, in its physical application, consists in its allowing us to ignore or leave out of account altogether the details of the mechanism, whatever it is, that is in operation in the phenomena under discussion; it makes everything depend on a single analytical function representing the distribution of energy in the medium in terms of suitable co-ordinates of position and of their velocities; from the location of this energy, its subsequent play and the dynamical phenomena involved in it are all deducible by straightforward mathematical analysis.

The problem of the correlation of the physical forces is thus divisible into two parts, (i) the determination of the analytical function which represents the distribution of energy in the primordial medium which is assumed to be the ultimate seat of all phenomena, and (ii) the discussion of what properties may be most conveniently and simply assigned to this medium, in order to describe the play of energy in it most vividly, in terms of the stock of notions which we have derived from the observation of that part of the interaction of natural forces which presents itself directly to our senses, and is formulated under the name of natural law. It may be held that the first part really involves in itself the solution of the whole problem; that the second part is rather of the nature of illustration and ex-

planation, by comparison of the intangible primordial medium with other dynamical systems of which we can directly observe the phenomena.

The chief representative of exact physical speculation of the second of these types has been Lord Kelvin. In the older attempts of this kind the dynamical basis of theories of the constitution of the æther consisted usually in a play of forces, acting at a distance, between ultimate elements of molecules of the medium; from this we must, however, except the speculations of Greek philosophy and the continuous vortical theories of the school of Descartes, which were of necessity purely descriptive and imaginative, not built in a connected manner on any rational foundation. It has been in particular the aim of Lord Kelvin to deduce material phenomena from the play of inertia involved in the motion of a structureless primordial fluid; if this were achieved it would reduce the duality, rather the many-sidedness, of physical phenomena to a simple unity of scheme; it would be the ultimate conceivable simplification. The celebrated vortex theory of matter makes the indestructible material atoms consist in vortex rings in a primordial fluid medium, structureless, homogeneous, and frictionless, and makes the forces between the atoms which form the groundwork of less fundamental theories consist in the actions excited by these vortices on one another through the inertia of the fluid which is their basis—actions which are instantaneously transmitted if the fluid is supposed to be absolutely incompressible.

In case this foundation proves insufficient, there is another idea of Lord Kelvin's by which it may be supplemented. The characteristic properties of radiation, which forms so prominent an element in actual phenomena, can be explained by the existence of an elastic medium for its transmission at a finite, though very great, speed; such a medium renders an excellent account of all its relations, if we assume it to possess inertia and to be endowed with some elastic quality of resistance to disturbance roughly analogous to what we can observe and study in ordinary elastic solids of the relatively incompressible kind, such as india-rubber and jellies. Lord Kelvin has been the promoter and developer of a view by which the elastic forces between parts of such a medium may be to some extent got rid of as ultimate elements, and be explained by the inertia of a spinning motion of a dynamically permanent kind, which is distributed throughout its volume. If we imagine very minute rapidly-spinning fly-wheels or gyrostats spread through the medium, they will retain their motion for ever, in the absence of friction on their axles, and they will thus form a concrete dynamical illustration of a type of elasticity which arises solely from inertia; and this illustration will be of great use in realising some of the peculiarities of a related

type, which I believe can be thoroughly established as the actual type of elasticity transmitting all radiations, whether luminous and thermal or electrical—for they are all one and the same—through the ultimate medium of fluid character of which the vortices constitute matter.

It has always been the great puzzle of theories of radiation how the medium which conveys it by transverse vibrations, such as we know directly only in media of the elastic-solid type, could yet be so yielding as to admit of the motion of the heavenly bodies through it absolutely without resistance. According to the view of the constitution of the æther which is developed in this paper, not only are these different properties absolutely consistent with each other, but it is, in fact, their absolute and rigorous coexistence which endows the medium with the qualities necessary for the explanation of a further very wide class of phenomena. The remark which is the key to this matter has been already thrown out by Lord Kelvin, in connexion with Sir George Stokes's suggested explanation of the astronomical aberration of light. The motion of the ultimate homogeneous frictionless fluid medium, conditioned by the motion of the vortices existing in it, is, outside these vortices, of an absolutely irrotational character. Now, suppose the medium is endowed with elasticity of a purely rotational type, so that its elastic quality can be called into play only by absolute rotational *displacement* of the elements of the medium; just as motion of translation of a spinning gyrostat calls into play no reaction, while any alteration of the absolute position of its axis in space is resisted by an opposing couple. As regards the motion of the medium involved in the movements of its vortices, this rotational elasticity remains completely latent, as if it did not exist; and we can at once set down the whole theory of the vortical hydrodynamical constitution of matter as a part of the manifestations of an ultimate medium of this kind.

We have now to indicate some of the consequences of the assumed constitution of the æther as regards the phenomena of radiation, which depend on this elasticity: to do this it will be convenient to make a fresh start, dealing more particularly with the first part of the general question.

The true nature of the phenomena of light had been brought to view at the beginning of the present century by the intuition of Thomas Young; and the secret of the exact quantitative mathematical laws which govern the behaviour of light in all the various circumstances attending its propagation, reflexion, and refraction had been fathomed in a marvellous manner by the genius of Fresnel. The nature of the mathematical reasoning by which Fresnel was led to his results has for the most part never been understood; and, as presented by him in his writings, it certainly seems devoid of

dynamical coherence and formal logical validity. Yet, the more the phenomena of light were afterwards experimentally examined, the stronger was the confirmation of the whole scheme of formulæ at which he had arrived.

The explanation of the laws of physical optics advanced by Fresnel, and verified by comparison with the phenomena, which was possible in several very exact ways, chiefly by himself and Brewster, was, about the year 1835, engaging the attention of several of the chief mathematicians of that time—Augustin Cauchy in France, Franz Neumann in Germany, George Green in England, and James MacCullagh in Ireland. The prevalent mode of attacking the problem was through the analogy with the propagation of elastic waves in solid bodies; and the comparison of Fresnel's laws of propagation in crystalline media with the results of the mathematical theory of the elasticity of crystalline bodies gave abundance of crucial tests for the verification, modification, or disproof of the principles assumed in these investigations. The treatment of Cauchy is earliest in date, but somewhat empirical and unsatisfactory in its logical aspects in the light of subsequent more precise knowledge of the conditions of the problem of the elasticity of solids. The treatment of Neumann is also a sound and original piece of investigation, if we except the limited view of the elasticity of solids, that of Navier and Poisson, on which he based it. The treatment by Green had the great distinction of incidentally laying, with all the generality and simplicity which we expect in an ultimate theory, the foundations on which every theory of elastic action in ordinary material bodies must in future be constructed; it proceeded, in fact, on the basis of one of those great generalisations, of which the aggregate constitutes the all-embracing modern doctrine of energy. These three authors all treated the question of reflexion and refraction of waves. Cauchy could not make much of Fresnel's formulæ in any logical manner. Neumann had the merit of seeing clearly that the thing was impossible on his elastic solid theory; so he dropped it altogether, assumed a sufficient number of principles which might be taken, with fair probability, in accordance with general reasoning, to be satisfied in the reflexion and refraction of light rays, viz., complete continuity of the media and continuity of energy in crossing the boundary at which the reflexion and refraction take place, and had the satisfaction of evolving a solution which agreed with Fresnel's laws, and easily extended them to the much more complicated circumstances of crystalline media. But to obtain this solution he assumed, from what he found necessary to make his very imperfect theory of propagation in crystals agree with Fresnel's laws, that the density of the luminiferous medium is the same in all bodies, and that the displacement of plane-polarised light is in the plane of polarisation. It may

be shown, as is now indeed to be expected, that this is a totally wrong foundation to work upon, that Neumann's general principles for the solution of the problem of reflexion are inconsistent with his elastic theory. If he had adopted a converse procedure, and worked out the problem of the reflexion of a ray on his general principles, and then deduced, by comparison with Fresnel's formulæ, the law of density of the luminiferous medium and the direction of the vibration in plane-polarised light, he would have been entitled to the credit of a joint discoverer in the domain of the dynamics of reflexion. But, for the reasons here indicated, the credit of that discovery must, I think, be assigned to MacCullagh.

The achievements by which the memory of MacCullagh is now to a great extent preserved are his very elegant investigations in the domain of pure Euclidian geometry. He may be claimed to be an instance of the numerous cases from Archimedes down through Descartes, Newton, and, we may add, Thomas Young, in which keen geometrical insight has formed a key for unlocking the formal laws of physical actions. He was first attracted to Fresnel's laws of optics by the very simple and elegant geometrical relations to which they lead. At a later period he proposed to himself the problem to hit off the extension of Fresnel's laws of reflexion which would apply to crystalline media, in the light of the crucial conditions afforded by the delicate experiments of Brewster and, at a later stage, Seebeck, to which such a theory must conform. He had thus to cast about for geometrical principles on which Fresnel's laws might be founded, such as would admit of easy extension to the more general problem. He early came upon the principle of continuity of the media, which he put in the geometrical form that the resultant of the displacements in the refracted waves is equal to the resultant of the displacements in the incident and reflected waves. As regards the other necessary condition, he was not at first successful. The density of the medium he took to be the same in all bodies, because he could not imagine it to be æolotropic, or different in different directions, in crystalline media. He assumed the vibrations to be in the plane of polarisation, from considerations of geometrical symmetry and necessity, confirmed in the earlier stage by one of the theories of Cauchy. The other condition above referred to he took to be equality of certain pressures in the media, as imagined by Cauchy; and by this means he arrived at a satisfactory explanation of Brewster's observations on the polarising angle in reflexion from crystals. But Seebeck pointed out that this solution would not account for the values of the deviation of the plane of polarisation from the plane of reflexion, by means of which he had himself tested it. Owing to this criticism MacCullagh was finally led to abolish Cauchy's notion of pressure, and assume simply the continuity of energy in its place.

This principle of energy, which gives a quadratic equation between the displacements at the interface, he succeeded to his satisfaction, as regards the confirmation of his views, in replacing by linear relations. And then he gave his two magnificent geometrical theorems—that of *transversals* and that of the *polar plane*, which contain each in a sentence the complete specification of the laws of reflexion for the most general case of a transparent medium, and which form the culmination of the geometrical relations by which he was guided throughout this whole process of synthetical discovery. His laws of reflexion are the same as Neumann's; of them, as formal laws, these two authors must be regarded as the independent discoverers—Neumann by a happy assumption suggested by reasoning at bottom illogical in the light of subsequent knowledge, MacCullagh by a resolute attack on the observed facts with a view to reducing them to simple formulæ.

But the greatest achievement of MacCullagh is that contained in his memoir of 1839, two years after, entitled an "Essay towards a Dynamical Theory of Crystalline Reflexion and Refraction." He is in quest of a dynamical foundation for the whole scheme of optical laws, which had been notably extended and confirmed by himself already. He recognises, I think for the first time in a capital physical problem, that what is required is the discovery of the potential-energy function of Lagrange on which the action of the medium depends, and that the explanation of the form of that function is another question which can be treated separately. His memoir is subsequent to, but apparently quite independent of, that of Green, in which Green restricted the medium to a constitution like an elastic solid, laid down the general laws of such constitution for the first time, and made a magnificent failure of his attempt to explain optical phenomena on that basis. If this thing was to be done, the power, simplicity, and logical rigour of Green's analysis might have been expected to do it; and nothing further has come of the matter until the recent new departure of Lord Kelvin in his speculation as to a labile elastic-solid æther. To return to MacCullagh, he is easily able to hit off a simple form of the potential-energy function, which—on the basis of Lagrange's general dynamics, or more compactly on the basis of the law of Least Action—absolutely sweeps the whole field of optical theory so far as all phenomena are concerned in which absorption of the light does not play a prominent part. He is confident, as any one who follows him in detail must be, that he is on the right track. He tries hard to obtain a dynamical basis for his energy-function, that is, to imagine some material medium that shall serve as a model for it and illustrate its possibility and its mode of action; he records his failure in this respect, but at the same time he protests against the limited view which would tie

down the unknown and in several ways mysterious and paradoxical properties of the luminiferous medium to be the same as those of an ordinary elastic solid.

The form of MacCullagh's energy-function was derived by him very easily from the consideration of the fact that it is required of it that it shall produce, in crystalline media, plane-polarised waves propagated by displacements in the plane of the wave front. Though he seems to put his reasoning as demonstrative on this point, it has been pointed out by Sir George Stokes, and is indeed obvious at once from Green's results, that other forms of the energy-function besides MacCullagh's would satisfy this condition. But the important point as regards MacCullagh's function is that it makes the energy in the medium depend solely on the absolute rotational displacements of its elements from their equilibrium orientations, not at all on its distortion or compression, which are the quantities on which the elasticity of a solid would depend according to Green.

Starting from this conception of rotational elasticity, it can be shown that, if we neglect for the moment optical dispersion, every crystalline optical medium has three principal elastic axes, and its wave-surface is precisely that of Fresnel, while the laws of reflexion and refraction agree precisely with experiment. Further, it follows from the observed fact of transparency in combination with dispersion, that the dispersion of a wave of permanent type is properly accounted for by the addition to the equations, therefore to the energy-function, of subsidiary terms involving spacial differentiations of higher order. To preserve the medium hydrodynamically a perfect fluid, these terms also must satisfy the condition that the elasticity of the medium is thoroughly independent of compression and distortion of its elements, and wholly dependent on absolute rotation. It can be shown, I believe, that this restriction limits the terms to two kinds, one of which retains Fresnel's wave surface unaltered, while the other modifies it in a definite manner stated without proof by MacCullagh [; but the first terms depend on an interaction between the dispersive property and the wave motion itself, while the second terms involve the square of the dispersive quality. It seems clear that the second type involves only phenomena of a higher order of small quantities than we are here considering—*December* 7, 1893]; thus an account of dispersion remains which retains Fresnel's wave surface unaltered for each homogeneous constituent of the light, while it includes the dispersion of the axes of optical symmetry in crystals as regards both their magnitudes and directions—results quite unapproached by any other theory ever entertained.

In this analysis of dispersions, all terms have been omitted which possess a unilateral character, such as would be indicated in actuality by rotatory polarisation and other such phenomena. The laws of

crystalline material structures seem to prohibit the occurrence of such asymmetry as these terms would indicate, except to the very small extent evidenced by the hemihedral faces of quartz crystals. The influence on the optical medium of this asymmetric arrangement of the molecules must be very much smaller still, for the rotatory terms are in all media exceedingly minute compared with the ordinary dispersional terms. The form of these rotatory terms in the energy-function is at once definitely assigned by our condition of perfect fluidity of the medium, both for crystals and for rotational liquids such as turpentine, and this form is the one usually accepted, on MacCullagh's suggestion, as yielding a correct account of the phenomena.

When dispersional terms are included in the energy function, our continuous analysis is not any longer applicable to the problem of reflexion; the conditions at the interface are altogether too numerous to be satisfied by the available variables. There is in fact discontinuity at the interface in the discrete molecular structure, such as could not be representable by a continuous analysis. But if we proceed by the method of rays, and assume that there is a play of surface forces which do not absorb any energy, while they adjust the dispersional part of the stress, it appears that reflexion is independent of dispersion.

The treatment of the problem of reflexion by Fresnel involves a different direction of vibration of the light, and different surface conditions, from MacCullagh's. It is of interest to remark that this theory may be stated in a dynamically rigorous form, provided the medium to which it refers possesses the properties of the labile elastic-solid æther of Lord Kelvin; and Fresnel's own account of his analysis of the problem becomes more intelligible from such a standpoint.

The mention of the phenomena of magnetic rotational quality will introduce us to the next division of the subject, that of the inclusion of electric and magnetic phenomena in the domain of the activity of this primordial medium.

The problem of the æther has been first determinedly attacked from the side of electrical phenomena by Clerk Maxwell in quite recent times; his great memoir on a 'Dynamical Theory of the Electromagnetic Field' is of date 1864. It is in fact only comparatively recently that the observation of Oersted, and the discoveries and deductions of Ampère, Faraday, and Thomson had accumulated sufficient material to allow the question to be profitably attacked from this side. Even as it is, our notions of what constitute electric and magnetic phenomena are of the vaguest as compared with our ideas of what constitutes radiation, so that Maxwell's views involve difficulties, not to say contradictions, and in places present obstacles

which are to be surmounted, not by logical argument or any clear representation, but by the physical intuition of a mind saturated with this aspect of the phenomena. Many of these obstacles may, I think, be removed by beginning at the other end, by explaining electric actions on the basis of a mechanical theory of radiation, instead of radiation on the basis of electric actions. The strong point of Maxwell's theory is the electromotive part, which gives an account of electric radiation and of the phenomena of electromagnetic induction in fixed conductors; and this is in keeping with the remark just made. The nature of electric displacement, of electric and magnetic forces on matter, of what Maxwell calls the electrostatic and the magnetic stress in the medium, of electrochemical phenomena, are all left obscure.

We shall plunge into the subject at once from the optical side, if we assume that dielectric polarisation consists in a strain in the æther, of the rotational character contemplated above. The conditions of internal equilibrium of a medium so strained are easily worked out from MacCullagh's expression for W, its potential energy. If the vector (f, g, h) denote the curl or vorticity of the actual linear displacement of the medium, or *twice* the absolute rotation of the portion of the medium at the point considered, and the medium is supposed of crystalline quality and referred to its principal axes, so that

$$W = \tfrac{1}{2}\int (a^2f^2+b^2g^2+c^2h^2)\, d\tau,$$

where $d\tau$ is an element of volume, it follows easily that for internal equilibrium we must have

$$a^2f\,dx+b^2g\,dy+c^2h\,dz = -dV,$$

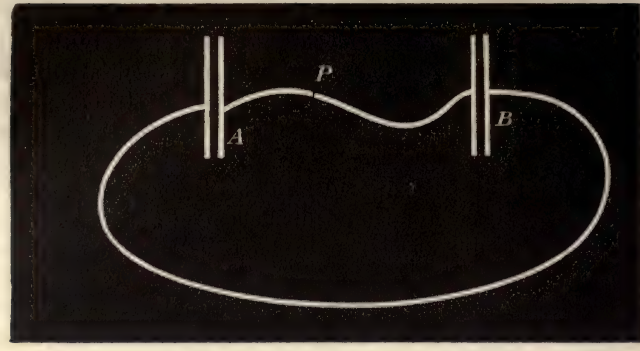
a complete differential, and that over any boundary enclosing a region devoid of elasticity the value of V must be constant. Such a boundary is the surface of a conductor; V is the electric potential in the field due to charges on the conductors; (f, g, h) is the electric displacement in the field, circuital by its very nature as a rotation, and (a^2f, b^2g, c^2h) is the electric force derived from the electric potential V. The charge on a conductor is the integral of (f, g, h) over any surface enclosing it, and cannot be altered except by opening up a channel devoid of elasticity, in the medium, between this conductor and some other one; in other words, electric discharge can take place only by rupture of the elastic quality of the æthereal medium.

[At the interface between two dielectric media, taken to be crystalline as above, the condition comes out to be that the tangential electric force is continuous. When the circumstances are those of equilibrium, and therefore an electric potential may be introduced, this condition allows discontinuity in the value of the potential in crossing

the interface, but demands that the amount of this discontinuity shall be the same all along the interface; these are precisely the circumstances of the observed phenomena of voltaic potential differences. The component, normal to the interface, of the electric displacement is of course always continuous, from the nature of that vector as a flux.

It may present itself as a difficulty in this theory that, as the electric displacement is the rotational displacement of the medium, its surface integral over any sheet should be equal to the line integral of the linear displacement of the medium round the edge of the sheet; therefore that for a closed sheet surrounding a conductor this integral should be null, which would involve the consequence that the electric charge on a conductor cannot be different from null. This line of argument, however, implies that the linear displacement is a perfectly continuous one, which is concomitant with and required by the electric displacement. The legitimate inference is that the electric displacement in the medium which corresponds to an actual charge cannot be set up without some kind of discontinuity or slip in the linear displacement of the medium; in other words, that a conductor cannot receive an electric charge without rupture of the surrounding medium; nor can it lose a charge once received without a similar rupture. The part of the linear displacement that remains, after this slip or rupture has been deducted from it, is of elastic origin, and must satisfy the equations of equilibrium of the medium. —*December* 7, 1893.]

We can produce in imagination a steady electric current, without introducing the complication of galvanic batteries, in the following manner, and thus examine in detail all that is involved, on the present theory, in the notion of a current. Suppose we have two charged condensers, with one pair of coatings connected by a narrow conducting channel, and the other pair connected by another such channel, as in the annexed diagram, where the dark regions are dielectric and the white regions conducting. If we steadily move towards each other the two plates of the condenser A, a current will flow round the circuit, in the form of a conduction current in the conductors and a displacement current across the dielectric plates of the condensers. Let us suppose the thicknesses of these dielectric plates to be excessively small, so as to minimise the importance of the displacement part of the current. There is then practically no electric force, and therefore no electric displacement, in the surrounding dielectric field, except between the plates of the condensers and close to the conducting wires. Consider a closed surface passing between the faces of the condenser A, and intersecting the wire at a place P. A movement of the faces of this condenser alters the electric force between them, and therefore alters the electric displacement across the portion of



this closed surface which lies in that part of the field; as we have seen there is practically no displacement, anywhere else in the field except at the conducting wire; therefore to preserve the law of the circuital character of displacement throughout the whole space, we must suppose that this alteration is compensated by a very intense change of displacement at the conducting wire. So long as the movement of the plates continues, as long does this flow of displacement along the wire go on; it constitutes the electric current in the wire. Now, in calculating the magnetic force in the field, which is the velocity of the æthereal medium, from the change of electric displacement, we must include in our integration the effect of this sheet of electric displacement flowing along the surface of the perfectly conducting wires, for exactly the same reason as in the correlative problem in hydrodynamics, of calculating the velocity of the fluid from the distribution of vorticity in it, Helmholtz had to consider a vortex sheet as existing over each surface across which the motion is discontinuous.

The next stage in this mode of elucidation of electrical phenomena is to suppose, once the current is started in our non-dissipative circuit, that both the condensers are instantaneously removed, and replaced by continuity of the wire. We are now left with a current circulating round a complete perfectly conducting channel, which in the absence of viscous forces will flow round permanently. The expression for the kinetic energy in the field is easily transformed from a volume integral of the magnetic force, which is represented by the velocity of the medium $\frac{d}{dt}(\xi, \eta, \zeta)$, to an integral involving the current $\frac{d}{dt}(f, g, h)$, which is in the present case a line integral round the electric circuit. The result is Franz Neumann's celebrated formula for the electromagnetic energy of a linear electric current,

$$T = \tfrac{1}{2}\iota^2 \iint r^{-1} \cos\epsilon \, ds \, ds;$$

or we may take the case of several linear circuits in the field, and obtain the formula

$$T = \tfrac{1}{2}\Sigma\iota^2\iint r^{-1}\cos\epsilon\, ds\, ds + \Sigma\iota_1\iota_2\iint r^{-1}\cos\epsilon\, ds_1\, ds_2,$$

which is sufficiently general to cover the whole ground of electrodynamics.

Our result is in fact that a linear current is a vortex ring in the fluid æther, that electric current is represented by vorticity in the medium, and magnetic force by the velocity of the medium. The current being carried by a perfect conductor, the corresponding vortex is (as yet) without a core, *i.e.*, it circulates round a vacuous space. [The strength of a vortex ring is, however, permanently constant; therefore, owing to the mechanical connexions and continuity of the medium, a current flowing round a complete perfectly conducting circuit would be unaffected in value by electric forces induced in the circuit, and would remain constant throughout all time. Ordinary electric currents must therefore be held to flow in incomplete conducting circuits, and to be completed either by convection across an electrolyte or by electric displacement or discharge across the intervals between the molecules, after the manner of the illustration given above.—*December* 7, 1893.]

Now we are here driven upon Ampère's theory of magnetism. Each vortex-atom in the medium is a permanent non-dissipative electric current of this kind, and we are in a position to appreciate the importance which Faraday attached to his discovery that all matter is magnetic. Indeed, on consideration, no other view than this seems tenable; for we can hardly suppose that so prominent a quality of iron as its magnetism completely disappears above the temperature of recalescence, to reappear again immediately the iron comes below that temperature; much the more reasonable view is that the molecular rearrangement that takes place at that temperature simply masks the permanent magnetic quality. In all substances other than the magnetic metals, the vortex atoms pair into molecules and molecular aggregates in such way as to a large extent cancel each other's magnetic fields; why in iron at ordinary temperatures the molecular aggregates form so striking an exception to the general rule is for some reason peculiar to the substance, which, considering the complex character of molecular aggregation in solids, need not excite surprise.

We have now to consider the cause of the pairing together of atoms into molecules. It cannot be on account of the magnetic, *i.e.*, hydrodynamical, forces they exert on one another, for two electric currents would then come together so as always to reinforce each other's magnetic action, and all substances would be strongly magnetic. The ionic electric charge, which the phenomena of electrolysis show to

exist on the atom, supplies the attracting agency. Furthermore, the law of attraction between these charges is that of the inverse square of the distance, and between the atomic currents is that of the inverse cube; so that, as in the equilibrium state of the molecule these forces are of the same order of intensity and counteract each other, the first force must have much the longer range, and the energy of chemical combination must therefore be very largely electrostatic, due to the attraction of the ions, as von Helmholtz has clearly made out from the phenomena of electrolysis and electrolytic polarisation.

But in this discussion of the phenomena of chemical combination of atoms we have been anticipating somewhat. All our conclusions, hitherto, relate to the æther, and are therefore about electromotive forces. We have not yet made out why two sets of molecular aggregates, such as constitute material bodies, should attract or repel each other when they are charged, or when electric currents circulate in them; we have, in other words, now to explain the electrostatic and electrodynamic forces which act between material systems.

Consider two charged conductors in the field; for simplicity, let their conducting quality be perfect as regards the very slow displacements of them which are contemplated in this argument. The charges will then always reside on their surfaces, and the state of the electric field will, at each instant, be one of equilibrium. The magnitude of the charge on either conductor cannot alter by any action short of a rupture in the elastic quality in the æther; but the result of movement of the conductors is to cause a re-arrangement of the charge on each conductor, and of the electric displacement (f, g, h) in the field. Now the electric energy W of the system is altered by the movement of the conductors, and no viscous forces are in action; therefore the energy that is lost to the electric field must have been somehow spent in doing mechanical work on the conductors; the loss of potential energy of the electric field reappears as a gain of potential energy of the conductors. We have to consider how this transformation is brought about. The movement of the conductors involves, while it lasts, a very intense ideal flow of electric displacement along their surfaces, and also a real change of displacement of ordinary intensity throughout the dielectric. The intense surface flow is in close proximity with the electric flows round the vortex atoms which lie at the surface; their interaction produces a very intense elastic disturbance in the medium, close at the surface of the conductor, which is distributed by radiation through the dielectric as fast as it is produced; the elastic condition of the dielectric, on account of its extreme rapidity of propagation of disturbances compared with its finite extent, being always extremely nearly one of equilibrium. It is, I believe, the reaction on the conductor of these wavelets which are continually shooting out from its surface, carry-

ing energy into the dielectric, that constitutes the mechanical forcive acting on it. But we can go further than this; the locality of this transformation of energy, so far at any rate as regards the material forcive, is the surface of the conductor; and the gain of mechanical energy by the conductor is therefore correctly located as an absorption of energy at its surface; therefore the forcive acting on the conductor is correctly determined as a surface traction, and not a bodily forcive throughout its volume. One mode of representing the distribution of this surface traction, which, as we know, gives the correct amount of work for every possible kind of virtual displacement of the surface, is to consider it in the ordinary electrostatic manner as a normal traction due to the action of the electric force on the electric density at the surface; we conclude that this distribution of traction is the actual one. To recapitulate: if the dielectric did not transmit disturbance so rapidly, the result of the commotion at the surface produced by the motion of the conductor would be to continually start wavelets which would travel into the dielectric, carrying energy with them. But the very great velocity of propagation effectually prevents the elastic quality of the medium from getting hold; no sensible wave is produced and no flow of energy occurs into the dielectric. The distribution of pressure in the medium which would be the accompaniment of the wave motion still persists, though it now does no work in the dielectric; it is this pressure of the medium against the conductor that is the cause of the mechanical forcive.

The matter is precisely illustrated by the fundamental *aperçu* of Sir George Stokes with regard to the communication of vibrations to the air or other gas. The rapid vibrations of a tuning-fork are communicated as sound waves, but much less completely to a mobile medium like hydrogen than to air. The slow vibrations of a pendulum are not communicated as sound waves at all; the vibrating body cannot get a hold on the elasticity of the medium, which retreats before it, preserving the equilibrium condition appropriate to the configuration at the instant; there is a pressure between them, but this is instantaneously equalised throughout the medium as it is produced, without leading to any flow of vibrational energy.

Now let us formally consider the dynamical system consisting of the dielectric media alone, and having a boundary just inside the surface of each conductor; and let us contemplate motions of the conductors so slow that the medium is always indefinitely near the state of internal equilibrium or steady motion, that is conditioned at each instant by the position and motion of the boundaries. The kinetic energy T of the medium is the electrodynamic energy of the currents, as given by Neumann's formula; and the potential energy W is the energy of the electrostatic distribution corresponding to the conformation at the instant; in addition to these energies we shall have to

take into account surface tractions exerted by the enclosed conductors on the medium, at its boundaries aforesaid. The form of the general dynamical variational equation that is suitable to this problem is

$$\delta\int(\mathrm{T}-\mathrm{W})\,dt+\int dt\int\delta w\,d\mathrm{S}=0,$$

where $\delta w\,d\mathrm{S}$ represents the work done by the tractions acting on the element $d\mathrm{S}$ of the boundary, in the virtual displacement contemplated. If there are electromotive sources in certain circuits of the system, which are considered to introduce energy into it from outside itself, the right-hand side of this equation must also contain an expression for the work done by them in the virtual displacement contemplated of the electric coordinates. Now this variational equation can be expressed in terms of any generalised coordinates whatever, that are sufficient to determine the configuration in accordance with what we know of its properties. If we suppose such a mode of expression adopted, then, on conducting the variation in the usual manner and equating the coefficients of each arbitrary variation of a coordinate, we obtain the formulæ

$$\Phi=\frac{d}{dt}\frac{d\mathrm{T}}{d\dot{\phi}}-\frac{d\mathrm{T}}{d\phi}+\frac{d\mathrm{W}}{d\phi},$$

$$\mathrm{E}=\frac{d}{dt}\frac{d\mathrm{T}}{d\dot{e}}.$$

In these equations Φ is a component of the mechanical forcive exerted on our dielectric system by the conductors, as specified by the rule that the work done by it in a displacement of the system represented by $\delta\phi$, a variation of a single coordinate, is $\Phi\delta\phi$: the corresponding component of the forcive exerted by the dielectric system on the conductor is of course $-\Phi$. Also E is the electromotive force which acts from outside the system in a circuit in which the electric displacement is e, so that the current in it is $\dot{e}$; the electromotive force induced in this circuit by the dielectric system is $-\mathrm{E}$.

These equations involve the whole of the phenomena of ordinary electrodynamic actions, whether ponderomotive or electromotive, whether the conductors are fixed or in motion through the medium: in fact, in the latter respect no distinction appears between the cases. They will be completed presently by taking account of the dissipation which occurs in ordinary conductors.

These equations also involve the expressions for the electrostatic ponderomotive forces, the genesis of which we have already attempted to trace in detail. The generalised component, corresponding to the co-ordinate ϕ, of the electrostatic traction of the conductors on the dielectric system, is $d\mathrm{W}/d\phi$; therefore the component of the traction,

somehow produced, of the dielectric system on the conductors is $-dW/d\phi$.

The stress *in the æther* between two electrified bodies consists of a tangential traction on each element of area, equal in magnitude to the tangential component of the electric force at that place, and at right angles to its direction. The stress in the *material* of the dielectric is such as is produced in the ordinary manner by the surface tractions exerted on the material by the conductors that are imbedded in it. The stress in the dielectric of Faraday and Maxwell has no real existence; it is in fact such a stress as would be felt by the surface of a conductor used to explore the field, when the conductor is so formed and placed as not to disturb the electric force in the dielectric. The magnetic stress of Maxwell is simply a mathematical mode of expression of the kinetic reaction of the medium.

The transfer of a charged body across the field with velocity not large compared with the velocity of electric propagation carries with it the whole system of electric displacement belonging to the body, and therefore produces while it lasts a system of displacement currents in the medium, of which the circuits are completed by the actual flow of charge along the lines of motion of the different charged elements of the body.

The phenomena of the electrostatic polarisation of dielectrics were at one time provisionally represented by Faraday as due to the orientation of electric polar elements of the medium by the electric force, just as magnetisation is actually due to the orientation of the magnetic polar elements by the magnetic force of the field; and this theory was developed at length by Mossotti. At a later period Maxwell himself ('Dynamical Theory,' § 11) compared the electric displacement in a dielectric medium to an actual displacement of the electric charge on conducting molecules imbedded in it—a conception mathematically equivalent to the above. In a previous paper* I have explained by simple reasoning that this view is inconsistent with the circuital character of the electric current, a conclusion in agreement with that of von Helmholtz, who adopted this idea in his generalised theory of electrodynamics. It is therefore necessary to obtain a complete view of this matter from our present standpoint. The polarised molecule, with its positive and negative ions, is as we have seen a reality; but if the current is to remain circuital, the action of the electric force of the field must not affect the actions between the constituent vortices which are the cause of their orientation, nor the distribution of the electric charges on the atoms, so much as to produce any sensible electric displacement of this kind. These restrictions might be secured by taking the two poles of the molecule

* "On the Theory of Electrodynamics," 'Roy. Soc. Proc.,' vol. 49, 1891, p. 522.

sufficiently close together, and by taking the dimensions of the conducting atom sufficiently small.

As regards the second of these hypotheses, it is to be observed that the moment of electric induction in a conducting atom depends only on its size, and not on the intensity of its free electrification; for the case of conducting spherules the electric moment produced by the action of an electric force F is $3F/4\pi$ multiplied by the total volume of the atoms, and this would give a dielectric inductive coefficient equal to three times the ratio of the aggregate volume of the atoms to the whole volume of the region, a result which is, in any case, far too small to represent the facts, and may easily be so small as to be quite negligible, so as to leave the current practically circuital.

But if we add on to this the first assumption, no room will be left for the explanation of the pyro-electricity and piezo-electricity of crystalline media, by changes of orientation of polar molecules due to changes of temperature or to applied pressure. If this very rational explanation is to be retained, we are driven to assume that the electric force of the field does not sensibly alter the orientation of a molecule, which would then be wholly controlled by the internal electrical, chemical, and cohesive forces of the medium. The state of matters thus required is, in fact, precisely realised by a symmetrical arrangement of positive and negative atoms in the molecule, such as the hexagonal molecule recently imagined by Lord Kelvin,* and earlier by J. and P. Curie, to account for the piezo-electric quality of quartz; the symmetry of the electric charges makes null the aggregate electric moment and therefore the turning couple in an electric field, while a differential polarity can still be developed under strain of the crystal.

According to the present theory of electrification, a discharge of electricity from one conductor to another can only occur by the breaking down of the elasticity of the dielectric æther along some channel connecting them; and a similar rupture is required to explain the transfer of an atomic charge to the electrode in the phenomenon of electrolysis. We can conceive the polarisation increasing by the accumulation of dissociated ions at the two electrodes of a voltameter, until the stress in the portion of the medium between the ions and the conducting plate breaks down, and a path of discharge is opened from some ion to the plate. While this ion retained its charge, it repelled its neighbours; but now electric attraction will ensue, and the one that gets into chemical contact with it first will be paired with it by the chemical forces; while if the conducting path to the electrode remains open until this union is complete, the ion will receive an opposite atomic charge from the electrode, which very conceivably may have to be also of equal amount, in order to equalise

* Lord Kelvin, 'Phil. Mag.,' October, 1893.

the potentials of the molecule and the plate. This is on the hypothesis that the distance between the two ions of a molecule is very small compared with the distance between two neighbouring molecules. A view of this kind, if thoroughly established, would lead to the ultimate averaging of atomic charges of all atoms that have been in combination with each other, even if those charges had been originally of different magnitudes. [The assignment of free electric charges to vortex atoms tends markedly in the direction of instability; though instability under certain circumstances is essential to electric discharge, yet it must not be allowed to become dominant.—*December* 7, 1893.]

The presence of vortex atoms, forming faults so to speak in the æther, will clearly diminish its effective rotational elasticity; and thus it is to be expected that the specific inductive capacities of material dielectrics should be greater than the inductive capacity of a vacuum. The readiness with which electrolytic media break down under electric stress may be connected with the extremely high values of their inductive capacities, indicating very great yielding to even a small electric force.

The radiation of a body into the surrounding medium is wholly electrical, and is due to the electric vibrations of the atomic charges; some of these types of vibration may correspond to the single atom by itself, while others will be considerably affected by the presence of the neighbouring atoms of the molecule. The most striking fact to be explained is the total independence of temperature that is exhibited by the periodic times corresponding to the various spectral lines. The extreme smallness of an atom implies correspondingly intense electrification, and therefore independence of the external field. If it is assumed that the dimensions and configuration of the atom are determined by the very intense actions between it and its partners in the molecule, and are not sensibly affected by the comparatively feeble influence of the velocity of translation of the molecule through the medium, this fact will be accounted for; irregularities can then only occur during an encounter with another molecule.

In the hydrodynamics of ordinary liquids, when the energy of an isolated vortex ring is increased, the ring expands in radius, and therefore moves onward more slowly. But in the case of an isolated charged atom, an expansion in radius diminishes the potential energy of the electric charge. These two agencies counteract each other; if the latter one is the greater, increase in energy will involve increase in velocity, as would be required in the ordinary form of the kinetic theory of gases. But the more natural supposition is, perhaps, to consider a molecule as composed of atoms paired so that the velocity of translation does not depend intrinsically on the amount

of energy associated with the molecule, but is determined by the circumstances of the encounters with other molecules. The distribution of energy between the various vibration-types of the molecule, according to the law of Maxwell and Boltzmann, will not affect its configuration, while there is also perfect independence between the hydrodynamical motion of the medium due to the molecule and the radiation produced by it.

As regards the rotational elasticity of this hydrodynamical æther, on which we have made all radiative and electrical phenomena depend, it was objected, in 1862, by Sir George Stokes* to MacCullagh's æther, that a medium of that kind would leave unbalanced the tangential surface-tractions on an element of volume, and therefore could not be in internal equilibrium; and this objection has usually been recognised, and has practically led to MacCullagh's theory of light being put aside, at any rate in this country. Now, it has been already mentioned that a precisely equivalent objection will apply to the elasticity actually produced by a gyrostatic distribution of momentum in an ordinary solid medium, the only difference in the circumstances being that in the latter case the rotational elasticity is proportional to the angular velocity and not to the angular displacement;† and this remark suggests that there must be some way out of the difficulty. If we consider the laws of motion, stated in Newton's manner with reference to absolute space and absolute time, as fundamental principles, then it is also a fundamental principle that the energy of a spinning gyrostat has reference to absolute space, and is not relative to the material system which contains it. The gyrostat may be considered as a kind of connexion binding that system to absolute immovable space by means of the forcive which it opposes to rotation; and this is the reason why the element of mass in a gyrostatic medium remains in equilibrium with its translational kinetic reactions, although the tractions of the surrounding parts on its surface are unbalanced and result in a couple. If this mode of viewing the subject is regarded as incongruous, then we must discard from dynamics the notion of absolute space, and we must set out in quest of some transcendental explanation of the directional forcives in rotational systems. In any case the general Lagrangian dynamical procedure applies precisely to the gyrostatic medium we have here taken as an illustration: nor, probably, would its application to MacCullagh's æther be questioned, once the preliminary objection was removed.

* [I am informed by Sir George Stokes that in the above criticism he contemplated only media of which the elements are self-contained, and devoid of internal motions.]

† For a detailed discussion of equilibrium and wave-propagation in such a medium, see 'Proc. Lond. Math. Soc.,' 1890.

This question may also be instructively illustrated from another side, by the consideration of an actual medium which possesses precisely the rotational elasticity of MacCullagh's æther. I allude to a solid medium with small magnets interspersed through it in any arbitrary manner, but so that in any single element of volume there is some regularity in their orientation. If this medium when unstrained is in equilibrium in a magnetic field, then when an element of it is displaced rotationally it will be acted on by a bodily couple arising from this external field; and therefore the surface tractions on the element would, in the presence of this couple, be unbalanced. Here the disturbing cause is a magnetic forcive arising either from the medium as a whole or from some external system; it has to be considered as of a statical character, that is, the velocity of propagation of the magnetic action is supposed to be indefinitely great compared with the velocity of propagation of any disturbances that are under discussion; the magnetic influence of the whole system is supposed to be instantaneously brought to bear on the element, and not merely the influence of the surrounding parts. On this saving hypothesis, the magnetic energy is here also correctly localised, for dynamical purposes, in the element of volume of the medium, and the Lagrangian method has perfect application to the mathematical analysis of its phenomena.

Now, in the case of the æther we have at hand a *vera causa* precisely of this kind. The cause of the phenomena of gravitation has hitherto remained perfectly inscrutable. Though the present order of ideas forbids us to consider it otherwise than as propagated in time, yet all we know of its velocity of propagation is the demonstration by Laplace that it must, at the very least, exceed the velocity of propagation of light in the same kind of proportion as the latter velocity exceeds that of ordinary motions of matter. It is not unphilosophical to assume that an explanation of gravitation might carry along with it the explanation of the fact that the tangential tractions on an element of the strained æther are unbalanced. The dynamical phenomena of mass in matter would appear to be analytically explicable by the addition of a rotational part to the kinetic energy of the element of the medium; such a term is of course practically null except in the vortex rings.

In all that has been hitherto said we have kept clear of the complication of viscous forces; but in order to extend our account to the phenomena of opacity in the theory of radiation and of electric currents in ordinary conductors, it is necessary to introduce such forces and make what we can of them on general principles. It is shown that the introduction of the dissipation function into dynamics by Lord Rayleigh enables us to amend the statement of the fundamental dynamical principle, the law of Least Action, so as to include

in it the very extensive class of viscous forces which are proportional to absolute or relative velocities of parts of the system. This class is the more important because it is the only one that will allow a simple wave to be propagated through a medium with period independent of its amplitude; if the viscous forces that act in light propagation were not of this kind, then on passing a beam of homogeneous light through a metallic film it should emerge as a mixture of lights of different colours. The viscous forces being thus proved by the phenomena of radiation to be derived from a dissipation function, it is natural to extend the same conclusion to the elastic motions of slower periods than radiations, which constitute ordinary electric disturbances. We thus arrive, by way of an optical path, at Joule's law of dissipation of electric energy, and Ohm's linear law of electric conduction, and the whole theory of the electrodynamics of currents flowing in ordinary conductors; though the presumption is that the coefficients which apply to motions of long period are not the same as those which apply to very rapid oscillations, the characters of the matter-vibrations that are comparable in the two cases being quite different.* If it is assumed that the form of the dissipation function is the same for high frequencies as for low ones, we obtain the ordinary theory of metallic reflexion, which differs from the theory of reflexion at a transparent medium simply by taking the refractive index to be a complex quantity, as was done originally by Cauchy, and later for the most general case by MacCullagh. And, in fact, we could not make a more general supposition than this for the case of isotropic media; while for crystalline media the utmost generality would arise merely from assuming the principal axes of the dissipation function to be different from those of the rotational elasticity, a hypothesis which is not likely to be required.

It has been pointed out, originally by Lord Rayleigh, that to fit this theory to the facts of metallic reflexion it is necessary to take the real part of the index of refraction of the metals to be a negative quantity, which can hardly be allowed on other grounds, as it would imply instability of the medium. We might indeed, following the view of Willard Gibbs and others, imagine an interaction between the light wave and the free vibrations of the atomic electric charges, and through them the chemical vibrations of the atoms, owing to proximity of their periods; and we might possibly conceive the electric medium to be, so to speak, held together by this kind of support. But I think there is another and simpler alternative that

* It is interesting to notice that, already in his memoir of 1864, Maxwell is struck by the identity of the coefficients of the free æther for all periods, which "shows how perfect and regular the elastic properties of the medium must be when not encumbered with any matter denser than air."

merits examination; we might conceive the opacity at the surface to be so great that a sensible part of the light is lost before it has penetrated more than a very small fraction of a wave-length. In the extreme case of electric waves of finite length reflected from metals, the absorption is complete in a very small fraction of the wave-length, and the result is total reflexion, as from a vacuum; on the other hand, if the opacity is but slight, the phenomena ought to agree approximately with those of transparent media. It seems worth while to examine the consequences of assuming that the optical phenomena of metallic reflexion are nearer the first of these limiting cases than the second. It seems worth while also to compare the facts for some medium not so opaque as metals with the formulæ of Cauchy and MacCullagh; the examples of tourmaline crystal, and some of the aniline dyes which exhibit selective absorption, suggest themselves as affording crucial tests.*

The considerations which have here been explained amount to an attempt to extend the regions of contact between three ultimate theories which have all been already widely developed, but in such a way as not to have much connexion with one another. These theories are Maxwell's theory of electric phenomena, including Ampère's theory of magnetism and involving an electric theory of light, Lord Kelvin's vortex-atom theory of matter, and the purely dynamical theories of light and radiation that have been proposed by Green, MacCullagh, and other authors. It is hoped that a sufficient basis of connexion between them has been made out, to justify a re-statement of the whole theory of the kind here attempted, notwithstanding such errors or misconceptions on points of detail as will unavoidably be involved in it.

[While writing this summary it had escaped my memory that Lord Kelvin has proposed a gyrostatic adynamic medium which forms an exact representation of a rotationally elastic medium such as has been here described.† If the spinning bodies are imbedded in the æther so as to partake fully in its motion, the rotational forcive due to them is proportional jointly to the angular momentum of a gyrostat and the angular velocity of the element of the medium, in accordance with what is stated above. But if we consider the rotators to be free gyrostats of the Foucault type, mounted on gymbals of which the outer frame is carried by the medium, there will also come into play a steady rotatory forcive, proportional jointly to the square of the an-

* [An alternative view, in many respects preferable, is supplied by the assumption, with Sir George Stokes, of the existence in metals of an *adamantine* property, such as was discovered by Airy for the diamond. *Cf.* Sir G. G. Stokes, 'Proc. Roy. Soc.,' February, 1883.]

† Lord Kelvin (Sir W. Thomson), 'Comptes Rendus,' Sept. 16, 1889; 'Collected Papers,' vol. 3, 1890, p. 467.

gular momentum of the gyrostat and to the absolute angular displacement of the medium. An ideal gyrostatic cell has been imagined by Lord Kelvin in which the coexistence of pairs of gyrostats spinning on parallel axles in opposite directions cancels the first of these forcives, thus leaving only a static forcive of a purely elastic rotational type. The conception of an æther which is sketched by him on this basis* is essentially the same as the one we have here employed, with the exception that the elemental angular velocity of the medium is taken to represent magnetic force, and in consequence the medium fails to give an account of electric force and its static and kinetic manifestations. A gyrostatic cell of this kind has internal freedom, and therefore free vibration periods of its own; it is necessary to imagine that these periods are very small compared with the periods of the light waves transmitted through the medium, in order to avoid partial absorption. The propagation of waves in this æther, having periods of the same order as the periods of these free vibrations, would of course be a phenomenon of an altogether different kind, involving diffusion through the medium of energy of disturbed motion of the gyrostats within the cells.

Lord Kelvin has shown that a fluid medium, in turbulent motion owing to vorticity distributed throughout it, would also possess rotational elasticity provided we could be assured of its permanence. Professor G. F. Fitzgerald proposes to realise such a medium by means of a distribution of continuous vortex filaments, interlacing in all directions; if the vorticities of the filaments in an element of volume are directed indifferently in all directions, the motional part of the kinetic forcive on the element, which depends on the first power of the vorticity, will be null, while the positional part depending on the square of the vorticity will remain, just as in the gyrostatic medium above considered. The atoms may now be imagined to consist of vortex rings making their way among these vortex filaments, and thus a very graphic and suggestive scheme is obtained; the question of stability is however here all-important. No ultimate theory can be final; and schemes of the kind discussed in this paper may not inaptly be compared to structural formulæ in modern chemistry; they bind together phenomena that would otherwise have to be taken as disconnected, though they are themselves provisional and may in time be replaced by more perfect representations.

The electric interpretation of MacCullagh's optical equations, which forms the basis of this paper, was first stated so far as I know by Professor G. F. FitzGerald, 'Phil. Trans.,' 1880. I have recently learned, from a reference in Mr. Glazebrook's Address, British Association, 1893, that an electric development of Lord Kelvin's

* Lord Kelvin (Sir W. Thomson), 'Collected Papers,' vol. 3, 1890, pp. 436—472.

rotational æther has been essayed by Mr. Heaviside, who found it to be unworkable as regards conduction-current, and not sufficiently comprehensive ('Phil. Trans.,' 1892, § 16; 'Electrical Papers,' vol. 2, p. 543). A method of representing the phenomena of the electric field by the motion of tubes of electric displacement has been developed by Professor J. J. Thomson, who draws attention to their strong analogies to tubes of vortex motion ("Recent Researches . . .," 1893, p. 52).

Professor Oliver Lodge has kindly looked for an effect of a magnetic field on the velocity of light, but has not been able to detect any, though the means he employed were extremely searching; the inference would follow, on this theory, that the motion in a magnetic field is very slow, and the density of the medium correspondingly great.—*December* 18, 1893.]

IV. "On Copper Electrolysis *in Vacuo*." By WILLIAM GANNON, M.A. Communicated by Professor SCHUSTER, F.R.S. Received November 14, 1893.

[Publication deferred.]

V. "Note on the Action of Copper Sulphate and Sulphuric Acid on Metallic Copper." By ARTHUR SCHUSTER, F.R.S. Received November 14, 1893.

[Publication deferred.]

VI. "On a Chart of the Symmetrical Curves of the Three-Bar Motion." By W. BRENNAND. Communicated by C. B. CLARKE, F.R.S. Received November 17, 1893.

Presents, December 7, 1893.

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December 14, 1893.

Sir JOHN EVANS, D.C.L., LL.D., Treasurer and Vice-President, followed by Professor J. S. BURDON SANDERSON and Sir G. M. HUMPHRY, in the Chair.

A List of the Presents received was laid on the table, and thanks ordered for them.

The Right Hon. James Bryce, a Member of Her Majesty's Most Honourable Privy Council, whose certificate had been suspended, as required by the Statutes, was balloted for and elected a Fellow of the Society.

The following Papers were read:—

I. "On the Constitution and Mode of Formation of Food Vacuoles in Infusoria, as illustrated by the History of the Processes of Digestion in *Carchesium polypinum*." By MARION GREENWOOD, Girton College, Cambridge. Communicated by J. N. LANGLEY, F.R.S. Received October 28, 1893.

(Abstract.)

Since the time that Ehrenberg first formulated his celebrated "polygastric" theory, there have been few writers on the Infusoria who have not confirmed his observations while combating the interpretation of them given by him. For it was shown long ago that the "Magenzellen" of Ehrenberg are spherical food masses, and these, circulating with varying rapidity in the "endoplasm," are so striking optically in most Infusoria that the literature of this group abounds in descriptions of or references to them. There is, however, a lack of any details which would throw light on the precise mode of origin of these ingesta, and yet this point has interest, for, while relatively large food masses are swallowed by certain of the ciliate Infusoria, very many forms are adapted only for the inception of minute solid particles, which seem far removed from the relatively large masses so noticeable within the animals.

An extreme case of this disparity between the size of ingested particles and the size of food masses circulating within the body is found in almost any member of the Vorticellidæ; the form I have chosen for examination is *Carchesium polypinum*, which grows in pedicellate clusters, each polype being mounted on a highly retractile

stalk, and being made up of relatively transparent cell substance. This animal shows such marked voracity upon occasion that I have counted 100 food masses within its substance; at the same time it is only minute particles which are acceptable when offered for ingestion. The shaping of the one form of matter from the other, then, promises to be a process of no small interest, and actual observation has made me think that it is more than merely interesting, that it may be regarded rather as *the striking illustration of a process often masked in other Protozoa, but fundamental in nature.*

I have fed *Carchesium* with nutritious and innutritious particles, with milk, with bacteria, with such flocculent precipitates as are thrown down by the interaction of ditch water and alizarin sulphate or congo-red, and with pigment grains—carmine, Indian ink, or ultramarine blue. All these are ingested readily, but the particles which perhaps serve best to illustrate the striking events of digestion are the finely-divided granules of proteid which form a precipitate when diluted white of egg is coagulated by heat. When the white of fresh eggs is treated thus, the heat precipitate is generally abundant; a very scanty coagulum may form, however, when the eggs from which the diluted fluid is prepared are stale. It is known that any marked alkalinity hinders effective coagulation of albumen and that albumoses and peptones do appear in stale white of egg; I think it fair, then, to suppose that *Carchesium* ingesting the abundant coagulation precipitate is ingesting minute irregular fragments of nutritious matter suspended in a dilute solution of salts, and that in administering the merely opalescent fluid a less obvious but possibly important substance is supplied—soluble food. All ingested particles, whatever their nature, pass from the exterior by a slightly sinuous ciliated pharyngeal tube which leads inwards from the wide mouth, and, spacious itself at first, narrows, to end internally in a small dilated sac, the œsophagus.* An anal ridge runs at right angles to the long axis of the polype at the junction of the outer and middle thirds of the pharynx, and from this ridge all effete matters are ejected, but food particles are gathered by the oral and pharyngeal cilia into the œsophageal sac, and, mixed in varying proportions with the fluid medium in which the animal is living, start from its most internal point on their intracellular career as a *vacuole of ingestion.*

Each vacuole of ingestion thus discharged by some obscure impulse performs a movement of *progression;* it passes with a steady gliding motion towards the basal attachment of the polype, coming to rest at some point along the concavity of the band-like nucleus. A period of *quiescence* follows and persists in healthy specimens for some seconds, and at its end the vacuolar contents, which up to this

* I have adopted the terminology of R. Greef ('Archiv für Naturgesch.,' Wiegmann, vol. 37, 1871).

point have been apparently in the condition in which they left the œsophagus, are rearranged in a very remarkable fashion. *The solid particles, it may be of proteid, of pigment, or bacteria, are gathered to a cluster, with a rapid centripetal movement; those which are more peripheral leave the boundaries of the vacuole, and a composite solid mass lies in clear fluid surroundings.* The outline of constituents so strikingly individual as are bacteria or the fat globules of milk may still be made out in the cohering cluster, but Brownian movement is ended, as are the "proper" movements of any small organisms which may be present, and further change is not in the direction of freedom, but makes the union closer; it tends to perfect the homogeneity of the composite solid. To this rearrangement of matter I apply the term "*aggregation*,"* for the obvious feature of the act is the clustering of particles of matter which were scattered before. It is most clearly demonstrable in vacuoles which contain but few minute particles suspended in a relatively large amount of water; it is masked when the solid matter preponderates or is less finely divided. No distinct relation can be traced, however, between the chemical character of the ingested matter and energy of aggregation, for nutritious and innutritious particles are moved with equal vigour and show equally little tendency to immediate subsequent separation. I might enumerate no inconsiderable number of variations of this process, some dependent on peculiarities of the ingesta dealt with (the presence of organic matter in solution, the rare enclosure of filamentous bacteria), some related rather to changes in the condition of *Carchesium* (abnormally eager ingestion or exceptionally lethargic action), but through all the modifications I have observed the salient characteristic of aggregation of solids and synchronous separation of fluid may be traced, and in face of each, the question arises, "*What force effects this movement and insures this redistribution of matter?*"

In answer to this question, three hypotheses may be considered:—

1. It may be supposed that as particulate proteid matter if pressed together with some force tends to form masses which cohere after the pressure is removed, so in the vacuole of ingestion the approximately symmetrical discharge of small jets of fluid from the surrounding protoplasm carries solid particles centripetally, and that, after displacement of the water between them, they cohere. It is noticeable, however, that grains of Indian ink may be united firmly in aggregation and discharged as a solid mass, that relatively large granules (such as the fat drops of milk) are inseparable after the first marked centripetal shifting,† and that nothing is more striking than

* I use this term with some reluctance in face of the fact that it was applied by Darwin many years ago ('Insectivorous Plants') to an entirely different process in the cells of the tentacles of *Drosera*.

† It will be gathered from what has been said above that only when these

the firm unbroken edge of a composite solid which abuts on the fluid of the vacuole in which it was formed. On these and other grounds this hypothesis appears to me inadequate, and I would lay but little stress upon it.

2. Again, it may be believed that, as in plasmolysed vegetable cells, the primordial utricle shrinks centripetally, gathering up in its retreat all granules which lie within or throughout its substance, so here *a highly elastic pellicle, living, or the product of secretion*, is set free from the walls of the vacuole and contracts rapidly, gathering within its lessening circumference all the solid particles which were suspended freely before. The unbroken line, which from the moment of *aggregation* marks off the clustered particles from the fluid in which they lie, does indeed suggest the presence of an enclosing film, but other experimental facts are clearly out of harmony with an hypothesis which postulates its existence. Thus the final cohesion of aggregated particles is at least as perfect in the centre of the composite mass as round its circumference; when by certain changes which may follow aggregation any particles are set free slowly, it is from the outside of the food mass only, and ejection from the body never means disintegration of the contents of an excretory vacuole, but is rather the freeing of a resistent solid. Further, there is, rarely, a want of synchronism in the aggregation of the particles in a vacuole; Brownian movement may persist for a time towards the end of the vacuole, when the majority of the granules present are quiescent; in other rare cases the aggregated mass clings to the cell substance from which it is (for the most part) separated by fluid, by slender threads of almost invisible, possibly mucilaginous, substance, these threads breaking presently and being dragged into the central solid; and it is usual to find that actual measurement of a food mass demonstrates the persistence of secondary shrinking after *aggregation* is over. I am inclined then to think that a third hypothesis meets the case more fully than either of those just mentioned, and to suggest—

3. *That the solid particles which undergo change of position in aggregation are dragged together by the comparatively rapid retraction of some substance contained in the vacuole; this substance is probably viscous.*

Such an hypothesis does not, however, offer any description of the mechanism of retraction, and the nature of this mechanism is certainly obscure. But I may point out that there are some undoubted resemblances between this rearrangement of substance and the phenomena which are grouped together as "clotting actions." In all perfected clots we have to recognise the interaction of two bodies or it may be the reconstitution of one body removed from the seat of most vigorous metabolic change—the cell, and a separation of solid

relatively large particles are present scantily in a vacuole do they move markedly in *aggregation.*

matter and subsequent shrinking, both varying in character and extent, are common accompaniments of the fundamental chemical reaction of clotting.

In *Carchesium*, then, it may be that we meet with a modification of the process, that there is an intravacuolar discharge of matter which clots, shrinking rapidly, and not with the slow change of casein or fibrin, and that it, entangling any solid particles which are present, brings about the spasm of *aggregation*. The substance is, indeed, not demonstrable usually by staining or other form of micro-chemistry, but delicate indicators of the presence of acid introduced into a digestive vacuole indicate that the vacuolar fluid begins to have an acid reaction about the time when *aggregation* is perfected. It is conceivable, then, that an access of acid fluid at this point may help the effective retraction of the clot, or even its first formation.

When by the process of aggregation spherical ingesta have been welded together in the substance of *Carchesium* from digestible or indigestible particles, they journey through the "endoplasm" in a fairly constant fashion, but for a variable time. Occasionally they are stored for hours after loss of the fluid of those vacuoles in which *aggregation* occurred; at times the digestion of nutritious matter follows the preliminary clustering with no marked pause. All nutritive ingesta present are not of necessity digested synchronously; indeed, there is sometimes apparent caprice in solution, but certain features of the process are invariable whenever its onset occurs.

Thus, as in *Amœba*, solution is effected in a *fluid medium*. The stored up food masses of *Carchesium*, when they have lost their fluid surroundings, have reached the extreme point of density and shrinkage; solution implies swelling, transparency, and *re-formation of a vacuole* if it has not persisted. Digestion may take place at any point throughout a relatively large part of the central substance of *Carchesium*, but complete solution is extremely rare, and innutritious remains travel with varying rapidity towards the anal ridge from which they are discharged. Thus they pass into the pharynx, to be swept to the exterior eventually by ciliary currents. It may be said that, other things being equal, the intracellular sojourn of ingesta tends to vary directly with their digestibility; thus clusters of aggregated particles in which such bodies as carmine or Indian ink preponderate or stand alone have a relatively short time of enclosure; the fluid of the vacuoles in which they are formed often disappears quickly, and there is but rarely (in the case of unmixed innutritious matter) that re-formation of fluid which is so nearly concerned in the solution of true food stuffs.

I have spoken hitherto with some vagueness of the duration of

successive events in the digestive process in *Carchesium*, and, indeed, the variability of many of them is marked. Changing conditions of the animal, on the one hand (and some of these changes cannot easily be controlled by the experimenter), and alteration in surroundings which may be bound up with temperature, aeration, illumination, or food, on the other, tend to bring out the elasticity of some of the periods which I have distinguished. But this elasticity has limits, nor does it characterise equally all the phases in the digestive cycle. We find that the total time of enclosure of innutritious matter may be as short as 30 minutes, that nutritious substances have a minimum (recorded) sojourn of 1 hour to $1\frac{1}{2}$ hour, and, on the other hand, that the time of enclosure may be prolonged to 30 hours. This great variation is found on examination to belong to that period in the history of ingesta in which they are stored, inert and destitute of fluid surroundings. The interval which separates successive acts of ingestion in any one series varies from 30 sec. to 65 sec., and is commonly 40 sec.; the duration of the movement of *progression* varies inversely (roughly speaking) with the duration of the phase of *quiescence.* Thus, progression may occupy $5\frac{1}{5}$ sec. or $14\frac{2}{5}$ sec., but is often $10\frac{2}{5}$ sec.; quiescence, with a usual duration of 9 sec., may be shortened to 5 sec., or lengthened to $25\frac{2}{5}$ sec. *Aggregation* is, as a rule, instantaneous in vigorous animals, but $\frac{2}{5}$ sec. or even $\frac{4}{5}$ sec. have passed between the onset and completion of the movement. The act of *solution* is more variable; I have seen very far reaching digestive change in 50 min., but that variation should be more striking than constancy is hardly surprising in face of the unlike nature of possibly digestible matter. Lastly, as I have said, the stage of *storage* may be omitted; in this case digestion succeeds *aggregation* at once, the fluid of the digestive vacuole increasing in amount and (presumably) changing in composition; on the other hand, ingesta may be stored for 22 hours before they are attacked by the true digestive secretion.

I have said above there seem to me to be grounds for regarding the aggregation of ingested particles in this complete and vigorous manner as a fundamental process in Protozoan digestion. Striking as the phenomenon is in *Carchesium*, the actual displacement of matter which it involves is, of course, small, and effective demonstration is possible chiefly because of the great transparency of the acting cell substance, and because the food is naturally, or may be kept artificially, in a state of minute division. Even in *Carchesium*, however, the marked retractility of the hyaline stalk of each polype often hinders observation; and, when it is remembered that so many Protozoa are vigorously motile, or relatively opaque, or deal (as do the Rhizopods) with comparatively massive food, it is hardly wonderful that a secretion of matter which is (by virtue of its own

characters) practically invisible, has occurred without exciting especial comment. R. Greef, in the paper to which I have referred, gives what I take to be a very brief description of the process of aggregation in *Epistylis flavicans*, and, looking back at observations made on the digestive processes in *Amœba* some time ago, I feel that many which were puzzling then are in harmony with the experimental results recorded above. Among these I may instance the very sudden quiescence after enclosure of such small organisms as monads, the firm union of unlike ingesta which were by chance enclosed together and so came to be in a common vacuole, and the cohesion after ingestion of particles of carmine or Indian ink.

And if further work should replace these scattered points of likeness by fuller, harmonious observations, then I think that the process of aggregation, owing the interest which it possesses, not to the obvious movement of particles, but to the more hidden mechanism which carries out the movement, may be allowed to have some such functional value as that indicated in *Carchesium* by the constancy of its duration and the constancy of its occurrence, whatever the chemical nature of the foreign particles involved. It may perhaps rank as an expression of what has been lacking among the Protozoa—what is clear enough among Cœlenterata, with their well-defined, unicellular glands—as an expression of *obscure histological change bound up with the digestion of food, or more nearly with its preparation for digestion.*

II. "The Action of Light on Bacteria. III." By H. Marshall Ward, D.Sc., F.R.S., Professor of Botany, Royal Indian Engineering College, Coopers Hill. Received December 14, 1893.

(Abstract.)

Several observers, notably Arloing, Janowsky, Geisler, and Chemelewsky, have tried to determine which rays of the spectrum are chiefly concerned in the destruction of bacteria, but all attempts hitherto have been made by placing separate tubes of broth, gelatine, or potato cultures in the various regions of the spectrum, and judging of the relative rates of growth by the periods in which turbidity is apparent, or by the sizes of the respective growths on solid cultures, and their conclusions vary considerably.

The author has succeeded in obtaining photographic records by throwing the spectrum on an agar film evenly charged with the spores or bacilli to be investigated, and then observing the behaviour of the illuminated regions after incubation.

Various species have been employed, *Bacillus anthracis*, *B. sub-*

tilis, a violet bacillus from the Thames, and several other Thames bacilli being the chief.

In all cases so far examined, both the solar and electric spectra show that no action whatever is perceptible in the infra-red, red, orange, or yellow region, while all are injured or destroyed in the blue and violet regions.

The exact point when the action begins and ends is not the same in all the experiments, though very nearly so, but it must be reserved for the detailed memoir to discuss the various cases.

Broadly speaking, the action begins at the blue end of the green, rises to a maximum as we pass to the violet end of the blue, and diminishes as we proceed in the violet to the ultra-violet regions (fig. 1).

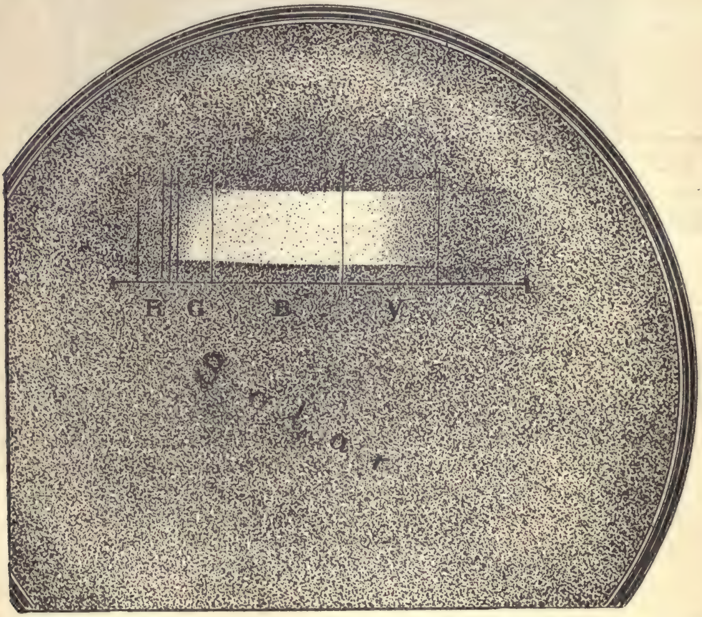

Fig. 1.—Plate of anthrax spores, exposed for five hours to the solar spectrum in August, and incubated for forty-eight hours. The spectrum shone on the plate through a slot of the width shown by the cleared portion, and whose length is denoted by the base line above the letters. The letters mark the principal regions of the spectrum; the vertical lines, the limits of these regions (not Fraunhofer's lines). Thus, those radiations we call infra-red, red (R), orange, and yellow affected the spores no more than total darkness, and colonies, therefore, germinated out in those regions as readily as over the main area of the plate. The same is partly true of the green (G) and the violet and ultra-violet regions to the right of V. The maximum effect is in the blue and blue-violet (BV), where nearly every spore has been destroyed, and the area appears cleared of colonies.

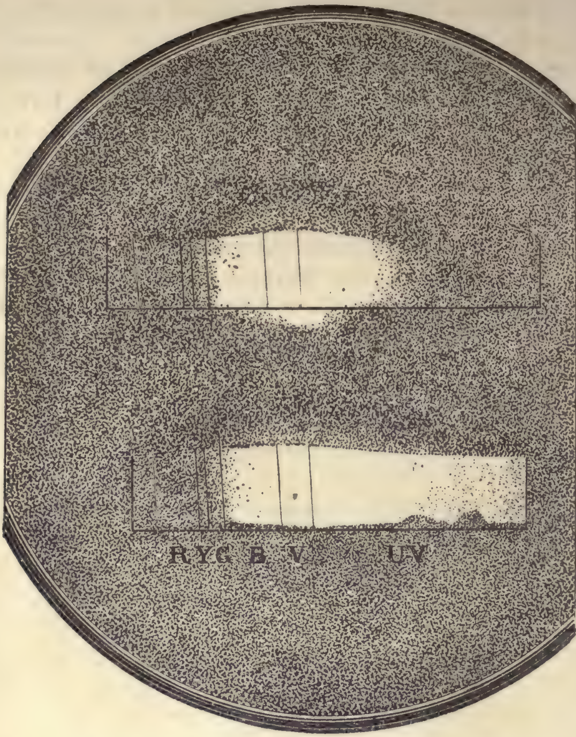

FIG. 2.—Plate similar to fig. 1, but exposed to the electric spectrum (obtained by means of quartz apparatus) for twelve hours, and incubated four days. The lower slot was covered with quartz only, the upper with a thin plate of glass. The base-line in each case gives length of exposed slot. In both cases the spores were uninjured in the infra-red, red (R), orange-yellow (Y), or green (G). The maximum effect was in the blue-violet, and it is interesting to see how the bactericidal action extended far into the ultra-violet (UV) in the case of the lower slot, where the light passed through quartz only. The two little protuberances over UV were due to two little overflows of burnt Canada balsam at the edge of the slot, cutting off light.

Some especially interesting results were obtained with the electric spectrum.* In the first place, the results with glass prisms,

* The author records his thanks to his colleagues, Professor Stocker and Mr. Shields, and to Drs. Woodhead and Cartwright Wood, for enabling him to try a few preliminary exposures to the electric lantern last winter and spring; these yielded no results, however, and it was not until he was so fortunate as to secure the hearty cooperation of Professor Oliver Lodge that it was possible to accomplish the photographing of the electric spectra in bacteria. To Professor Lodge and Mr. E. Robinson the author takes this opportunity of expressing his special thanks for the continuous pains they have taken to have his plates properly exposed. The

lenses, &c., were so feeble that it was necessary to employ quartz throughout.

Secondly, the bactericidal effect is found to extend far into the ultra-violet. The intervention of a thin piece of glass results in the cutting off of a large proportion of effective rays (fig. 2).

Thirdly, the most destructive rays—end of blue and beginning of violet—are to some extent effective even after reflection from the inner faces of the quartz plate covering the film and the glass on which it is supported, and so a peculiar bellying out of the image of the illuminated slot is observable during the early stages of incubation—the figure being thus made to show its own curve of intensity as it were.

The plates employed are ordinary agar cultures in shallow glass dishes, covered with a glass plate in which one or more slots—about 3 ins. long by $\frac{1}{2}$ in. wide—are pierced. Over the slots a quartz plate is secured, and all covered with black paper and foil, except the slots. The exposures are made on ice.

The author is also using plates divided in two halves, so that two similar films containing bacteria of different species can be exposed simultaneously to the same spectrum.

These results suggest evidently that the naked arc light may prove to be a very efficient disinfecting agent in hospital wards, railway carriages, or anywhere where the rays can be projected directly on to the organism. The author has elsewhere shown the evidence on which it is concluded that the action is direct and on the cell contents; but even if the action took place at the surface of the cells, the above conclusion would still be true in practice.

It is extremely desirable that experiments should be made on the action of light on living cells of animals—*e.g.*, Infusoria, ova, &c—since results would probably be obtained of importance as regards sunburn, sun-baths, and other matters.*

exposures to the solar spectra were made by the author himself, and he is indebted to Professors McLeod and Stocker for the use of apparatus and for valuable advice.

* Raum, in 'Zeitschr. f. Hygiene,' 1889, has collected some literature bearing on this subject.

III. "A Record of Experiments illustrative of the Symptomatology and Degenerations following Lesions of the Cerebellum and its Peduncles and related Structures in Monkeys." By DAVID FERRIER, M.D., F.R.S., Professor of Neuropathology, and W. ALDREN TURNER, M.D., Demonstrator of Neuropathology, King's College, London. Received November 30, 1893.

(From the Neuropathological Laboratory, King's College, London.)

(Abstract.)

This paper is the detailed record of the symptoms, temporary and permanent, following total and partial extirpation of the cerebellum, and section of its peduncles, and the degenerations so induced; and includes the effects of destruction of the tubercles on the posterior surface of the medulla oblongata, and the degenerations resulting therefrom, together with some observations on the central relations of the 5th cranial nerve. The paper is illustrated by photographs taken direct from the microscopical sections. Special reference is made to the similar researches of Luciani and Marchi.

The most noteworthy features of complete extirpation of the cerebellum were the extraordinary disturbances of station and locomotion, and the long-continued and apparently persistent unsteadiness of the trunk and limbs on muscular effort. There were noted, also, from the first, absence of tonic flexion or contracture of the limbs; retention of great and, apparently, unimpaired muscular strength, as evidenced by the firmness of the grasp of the bands and feet, and the agility in climbing; and the presence, with ultimate exaggeration, of the knee-jerks. There was no impairment of the general or special sensibility, or disturbance of the organic functions.

The symptoms observed after extirpation of a lateral lobe, after the first tumultuous disturbance of equilibrium had passed off, were similar to those observed after complete extirpation, with the important difference that they were confined to the limbs on the side of lesion. Except in one case, where it was only present to a slight extent, there was no impulsive tendency to rotation.

Extirpation of the middle lobe, including antero-posterior division, produced, in general, the same symptoms as were observed in connexion with removal of the whole organ and of the lateral lobe, but they did not affect one side more than the other, and were more pronounced in the head and trunk than in the limbs.

The symptoms following section of the cerebellar peduncles were similar to those occurring after removal of the lateral lobe, the chief

difference being the greater tendency to roll round the longitudinal axis towards the side of lesion, whichever peduncle was cut.

Destruction of the clavate and cuneate nuclei caused temporary disturbances of attitude and gait, but there was no affection of cutaneous sensibility.

The degenerations following removal of the lateral lobe of the cerebellum, or section of the superior peduncle, showed that this structure contains an efferent tract to the opposite red nucleus and optic thalamus, and an afferent tract, which appears to be the cerebellar termination of the antero-lateral ascending tract of Gowers.

Lateral lobe extirpation, or section of the middle peduncle, was followed by diminution of the transverse fibres of the pons Varolii on the side of the lesion, and atrophy of the cells of the nucleus pontis on the opposite side.

Lateral lobe extirpation, or section of the inferior peduncle, demonstrated the existence of an efferent tract to the opposite inferior olivary body, and of an afferent tract to the cortex, chiefly of the lateral lobe.

Extirpation of the middle lobe occasioned no degeneration in the superior, middle, or inferior cerebellar peduncles, but was followed by degeneration and sclerosis of the tract which passes from the vermiform process to Deiters' nucleus—the "direct sensory cerebellar tract" of Edinger.

We were unable to confirm Marchi's statements as to the existence of a direct efferent cerebellar tract in the spinal cord, or of degeneration in the anterior nerve roots, mesial fillet, or posterior longitudinal bundles, after cerebellar extirpation.

In two cases of lateral lobe extirpation, however, we obtained degeneration in the anterior and lateral columns of the spinal cord respectively, in the position indicated by Marchi. In the case, however, in which there was a marginal degeneration in the anterior column, the nucleus of Deiters, on the same side, was implicated; while, in that in which degeneration in the lateral column was present, there was a lesion of the tegment of the pons, involving the nucleus of the lateral fillet. The same degeneration was induced by lesions specially made in the lateral fillet.

Destruction of the clavate and cuneate nuclei was followed by degeneration, on the one hand, through the restiform body into the cerebellum; and, on the other hand, through the internal and middle arcuate fibres to the opposite interolivary layer and mesial fillet. This latter structure was traced to the anterior quadrigeminal bodies and optic thalamus.

Owing to lesion in some of the experiments of the roots of the 5th cranial nerve, we were led to make special investigations on its cen-

tral connexions. Degeneration and sclerosis of the so-called "ascending root" was traced as far as the 2nd cervical nerve, after section of the sensory division; and atrophy of the so-called "descending root" was observed after section of the motor division.

We were unable to confirm the existence of a direct cerebellar root to this nerve.

IV. "On the Relations of the Nucleus to Spore-formation in certain Liver-worts." By J. BRETLAND FARMER, M.A., Royal College of Science, London. Communicated by Professor VINES, F.R.S. Received November 9, 1893.

It is well known that, as a general rule, during the formation of spore tetrads from their mother cells, the nucleus of the latter commonly undergoes two successive bipartitions. Each of the resulting four nuclei ultimately becomes a centre for the aggregation of a portion of the original protoplasm, whilst division of the whole immediately follows by means of cell walls.

Though the above method is the one most commonly followed during the process of spore development, so far as the essential features are concerned, it is by no means the invariable one. Probably, however, it is to be regarded as typical, and the deviations about to be described should be interpreted as modifications of it.

Anyone who is familiar with Hofmeister's drawings, or who has ever seen spore production actually going on in the Hepaticæ, must have noticed that, in many species, the mother cell of the tetrad becomes four-lobed previously to its breaking up into its four spores. This lobed appearance is seen whilst the original nucleus is still resting, and is due to a bulging out of the cell wall in four directions, accompanied by an ingrowth of cellulose into the lumen of the cell, and towards the nucleus.

If the process be followed in *Aneura multifida*, the intruding walls are seen to closely approach the nucleus while this body is still in the resting state. The latter body then divides very rapidly, forming first ten, then twenty, chromosomes, which are arrayed along a very short spindle at the centre of the cell. Then another spindle appears in a plane inclined to that of the first, and the number of the chromosomes is apparently about forty, though, by reason of their small size and the difference in their planes, it is difficult to be quite certain as to their number. The nucleus here then goes through the ordinary form of karyokinesis, but in a somewhat compressed form. The four groups of ten chromosomes then move off along the achromatic spindles to their respective lobes, and the further ingrowth of the cell walls to the centre, where they meet, cuts off the several proto-

plasmic portions of the four spores. The achromatic spindle is fairly well marked, and runs up sharply to a point in each lobe, and a structure can often be seen at the end which possibly represents the presence of a centrosome.

In *Aneura pinguis* the case is somewhat different. The lobing of the spore mother cell is much more apparent than in *A. multifida*, and just before division occurs, a portion of the dense protoplasm which surrounds the nucleus (and which should probably be regarded as an archoplasm) protrudes as an achromatic spindle simultaneously into *each* of the four cell lobes. This takes place while the nucleus is still resting, and thus a quadripolar spindle is formed. The nucleus then becomes also lobed, in such a way that it assumes a tetrahedral shape, with an angle in contact with each achromatic sheaf. I have not, through scarcity of material, as yet been able to observe the behaviour of the chromosomes in this species, but there is hardly room for doubt that a simultaneous separation of the bodies into four groups takes place. This conclusion is strongly supported by the peculiar behaviour of the archoplasmic spindle already described. It should be mentioned that the application of such stains as gentian-violet and orange, or hæmatoxylin and orange, gives such clear results that it is impossible to mistake the character of the process.

The most conclusive results, as well as the most remarkable, were obtained in the case of a tropical Liver-wort, *Steetzia decipiens*, which I collected when in Ceylon two years ago. The division of the nucleus takes place at an extremely late period in the development of the spores. The intruding walls grow into the cavity of the mother cell to such an extent as almost to touch the nucleus itself, whilst the sculpturing so characteristic of the mature spore is quite obvious, even before the nucleus exhibits any sign of approaching division. I have been fortunate in securing a large number of preparations in which all the steps are clearly shown, and have further succeeded in getting the important stages photographed. The nucleus is invested by a dense mass of protoplasm (archoplasm), and this, as in *Aneura pinguis*, forms a quadripolar achromatic spindle whilst the nucleus is still in the resting condition. The ends of the spindle severally reach out to a point beyond the centre of each lobe. I have termed the spindle *achromatic*, following the common usage, but, as a matter of fact, it stains deeply with hæmatoxylin, safranin, or gentian-violet—a point of some interest. The chromatic portion of the nucleus now forms a large mass in the centre, and becomes *four-lobed*, the lobes being united centrally till quite late.

Then they become separate, and each breaks into two chromosomes of a rod-like shape, which are at first aggregated somewhat irregularly in the centre, from which the achromatic spindles diverge. Speedily, however, they arrange themselves in pairs, and each pair

furnishes the chromatic element to the daughter nucleus. The cell walls rapidly meet in the centre, and their union is effected before the reconstruction of the daughter nuclei. The spindle mass contracts up to the middle of each of the four cells, and invests the young nucleus in the same manner as was the case with the original body.

Often during this part of the process there appeared to be two nuclei in each spore cell, but I regard this as probably due to an unequal contraction of the archoplasmic mass. In many of the daughter nuclei the two chromosomes could be detected for some little time, but they frequently become more numerous, and they finally lose their distinctness and are impossible to trace, though I leave, for the present at least, the question of their real permanence an open one.

[Since the above was written, I have seen a quadripolar spindle, also in *Aneura multifida*. It is not so well marked as in *A. pinguis*, and seems only to occur immediately before division. Its extremely short duration is indicated by the fact that, although I possess several hundred preparations, all fixed at nearly the same stage, in only two of them is there unequivocal evidence of the existence of such a spindle *before* the individualising of the chromosomes takes place.—November 21, 1893.]

V. "Sugar as a Food in the Production of Muscular Work." By VAUGHAN HARLEY, M.D., Teacher of Chemical Pathology, University College, London, Grocer Research Scholar. Communicated by GEORGE HARLEY, M.D., F.R.S. Received November 22, 1893.

It may be said to have been universally believed that proteids were the essential producers of muscular work until the experiments of Voit* and Pettenkofer† showed that, within certain limits, muscular work can be produced by carbohydrates. They did this by showing the relative amounts of nitrogen eliminated during muscular activity and repose. Subsequently, Chauveau and Kaufmann‡ showed, by comparing the quantity of sugar that disappeared from the blood traversing a muscle while contracting and at rest, that four times more sugar was used up during the period of muscular activity. Having failed to find any further recorded facts regarding sugar as a muscle food, I thought it desirable, in connexion with the investiga-

* Voit, 'Ueber d. Einfluss d. Muskelbewegung auf d. Stoffwechsel.' München, 1860.

† Pettenkofer and Voit, *ibid.*, vol. 2, p. 459, 1866.

‡ Chauveau and Kaufmann, 'Compt. Rendus,' vol. 103, 1886; vol. 104, 1887.

tion I have for some time been engaged upon, as regards the *rôle* played by sugar in the animal organism, to try and ascertain, by direct experiment, if sugar when taken as food is actually a supporter of muscular energy—a point of great value to be decided at the present moment, when sugar is so cheap that its use need no longer be restricted to that of a mere palatable condiment, but it might, perhaps, be profitably added to the daily diet of the working man as a muscular power-producing element.

With the object of, if possible, settling this point, I availed myself of the opportunity I had of making a series of experiments upon myself with Professor Mosso's ergograph, while working in the autumn of 1892 in the Physiological Laboratory at Turin.*

The amount of muscular energy developed by sugar was calculated by the quantity of work that could be done by the muscles of the middle finger of each hand, in a given time, before fatigue set in. And I think that the results obtained by experimenting with the fingers may not unreasonably be regarded as a reliable indication of the effects of sugar on the other muscles of the body.

Throughout the whole time of the experiments, except when it is specially mentioned to the contrary, not only was the mode of life, as regards the amount of sleep, &c., kept uniform and the same kind of food taken, but, as nearly as possible, in the same quantities, along with varying amounts of sugar taken.

Each separate experiment with the ergograph was repeated every two hours, a voluntary muscular contraction being made every two seconds. Being right-handed, a 4-kilo. weight was used for the right finger and a 3-kilo. one for the weaker left.

The total height to which the weight was raised, being multiplied by the weight, expressed in kilogrammetres the amount of work accomplished.

The amount of work done was calculated by two methods: firstly, the total amount of work accomplished up till fatigue set in; secondly, the amount of work accomplished by each thirty voluntary muscular contractions. The diurnal variations in the amount of work performed, as pointed out by Lombard,† and confirmed in my own experiments, rendered it necessary to compare the results at precisely the same hours on different days, in order to avoid error in drawing conclusions of the value of sugar, as the muscular working capacity varies at different times of the day.

The first step was to ascertain the value of sugar when taken alone

* I here beg to express my warmest thanks to Professor Mosso for kindly placing his apparatus at my disposal, and I may at the same time mention that his brother Professor Ugolino Mosso and Luigi Paoletti afterwards corroborated the results I obtained ('Report of the Roman Academy,' 15th October, 1893).

† Warren Lombard, 'Journal of Physiol.,' vol. 13, p. 1, 1892.

in the production of muscular work. During a twenty-four hours' fast—on one day water alone was drunk, on another day 500 grams of sugar was taken in an equal quantity of water.

Table showing the Increase in the Muscular Power of 30 Voluntary Contractions produced by 500 grams ($17\frac{1}{2}$ ounces) of Sugar.—200 grams (7 ounces) being taken at 8.30; 100 ($3\frac{1}{2}$ ounces) at 11 A.M., at 2 P.M., and 5 P.M.

Time of day.	Fasting.			Fasting + sugar.	
	Hand.	Weight raised, kilos.	Kilogram-metres by 30 contractions.	Kilogram-metres by 30 contractions.	Gain in work from sugar in kilogram-metres.
9.30 A.M.	Left	3	3·372	4·452	+1·080
11.30 ,,	,,	,,	3·273	4·812	+1·539
1.30 P.M.	,,	,,	3·513	4·524	+1·011
3.30 ,,	,,	,,	3·735	3·798	+0·063
5.30 ,,	,,	,,	3·186	3·873	+0·687
Total	,,	,,	17·079	21·459	+4·380 = 25·646 p. c.
11.20 A.M.	Right	4	3·112	3·552	+0·440
1.45 P.M.	,,	,,	2·892	3·200	+0·308
3.40 ,,	,,	,,	3·240	3·860	+0·620
5.45 ,,	,,	,,	2·300	4·704	+2·404
Total	..	..	11·544	15·316	+3·772 = 32·675 p. c.

The above table shows that sugar when taken alone increases the amount of muscular work done, as shown by thirty voluntary contractions. The left middle finger accomplished, during five periods, 21·459 kilos. of work, as against 17·079 kilos. on the fasting day, showing an increase in the muscle-working power, on sugar, of 25·646 per cent. The right hand, during the four periods when sugar was taken, performed 15·316 kilos. of work, as against 11·544 kilos. while fasting, thus showing the still higher gain in muscular power, on sugar, of 32·675 per cent.

The effect of sugar in retarding the approach of fatigue was next estimated.

Table showing the Power 500 grams ($17\frac{1}{2}$ ounces) of Sugar has of retarding Fatigue, as well as the total Increase in Muscular Power it produces.—200 grams (7 ounces) being taken at 8.30 A.M., and 100 grams ($3\frac{1}{2}$ ounces) at 11 A.M. and at 2 and 5 P.M.

	Fasting.		Fasting + sugar.		
Time of day.	Time during weight lifted, seconds.	Total work, kilogram-metres.	Time during weight lifted, seconds.	Total work, kilogram-metres.	Gain in work, kilogram-metres.
Left middle finger raising a weight of 3 kilos.					
9.30 A.M.	78	3·408	94	5·949	+ 2·541
11.30 „	76	3·816	106	9·627	+ 5·811
1.30 P.M.	84	4·614	92	5·979	+ 1·365
3.30 „	66	4·245	118	6·381	+ 2·136
5.30 „	70	3·342	118	6·375	+ 3·033
Total...	374	19·425	528	34·311	+ 14·886 = 76 p. c.
Right middle finger raising a weight of 4 kilos.					
11.20 A.M.	88	4·028	100	4·080	+ 0·052
1.45 P.M.	78	3·380	114	4·376	+ 0·996
3.40 „	72	3·648	132	6·680	+ 3·032
5.45 „	62	2·420	138	6·564	+ 4·144
Total...	300	13·476	484	21·700	+ 8·224 = 61 p. c.

It is here seen that the 500 grams ($17\frac{1}{2}$ ounces) of sugar retarded the onset of fatigue by more than 150 seconds in both cases. The left middle finger being capable of working 528 seconds, as against 374 seconds; while the right worked 484 seconds, as against 300 seconds.

The total work performed by the left middle finger was 34·311 kilos. as against 19·425 kilos., giving a total gain of 76 per cent.; the right, 21·700 kilos. as against 13·476 kilos., yielding a total gain of 61 per cent. from the sugar.

When the result obtained from each successive working of the ergograph is compared, the total muscle-working power is apparently far more increased than that obtained from merely thirty contractions. This, in all probability, is due to the carbohydrates formed during fasting, from the proteids of the tissues, being sufficient for the performance of a small amount of work, though the supply is insufficient for prolonged work. When, however, sugar is taken, it supplies the

muscles with a sufficiency of working material, and the advantage, consequently, is most markedly seen in the total amount of work performed.

Having found that sugar when taken by itself is undoubtedly a muscular food, it was next necessary to ascertain its value when it is taken along with and in addition to an ordinary diet.

First, in order to see the effects of sugar added to a frugal meal, 200 grams (7 ounces) of sugar were taken at 9 A.M., after a breakfast, at 8 A.M., of a cup of coffee, with milk, and two rusks.

Table showing Gain in Muscular Work obtained by adding 200 grams (7 ounces) of Sugar to a Frugal Breakfast.

	On coffee, milk, and rusks.		On coffee, milk, and rusks + sugar.		
Time of day.	Time during weight lifted, seconds.	Total work, kilogram-metres.	Time during weight lifted, seconds.	Total work, kilogram-metres.	Gain in work, kilogram-metres.
Left middle finger raising a weight of 3 kilos.					
9.30 A.M.	112	5·711	110	5·733	+0·022
11.30 ,,	122	6·477	124	7·206	+0·729
Total...	234	12·188	234	12·939	+0·751 = 6·162 p. c.
Right middle finger raising a weight of 4 kilos.					
9.40 A.M.	100	6·312	112	7·108	+0·796
11.40 ,,	122	7·556	180	12·176	+4·620
Total...	222	13·868	292	19·284	+5·416 = 39·06 p. c.

Here it is seen that 200 grams (7 ounces) of sugar increased the amount of work performed, both by the left and right hand. The increase in the quantity of work done being most marked in the case of the right finger, for it was 39·06 per cent., whereas the left was only 6·16 per cent. It is further seen that, although the taking of the sugar caused an immediate increase in the work done, the increase was far more marked two and a half hours later; that is to say, when its assimilation had taken place.

Having thus ascertained that sugar increases the power of doing muscular work when added to a small meal, I will now give an example of what it does when added to a full one; that is to say, a luncheon consisting of beefsteak with vegetables, an omelet, and

bread. along with a quarter bottle of red Italian table wine, and after it a small cup of black coffee.

In this case 250 grams ($8\frac{1}{2}$ ounces) of sugar were taken along with the meal at 12.30.

Table showing Gain in Muscular Work caused by the addition of 250 grams of Sugar to a Full Meal.

Time of day.	Luncheon.		Luncheon + sugar.		
	Time during weight lifted, seconds.	Total work, kilogram-metres.	Time during weight lifted, seconds.	Total work, kilogram-metres.	Gain in work, kilogram-metres.
Left middle finger lifting a weight of 3 kilos.					
2 P.M.	140	7·902	140	9·168	+1·266
4 "	130	7·533	150	8·511	+0·978
6 "	112	6·795	142	8·241	+1·446
Total ...	382	22·230	432	25·920	+3·690 =16·599 p. c.
Right middle finger lifting a weight of 4 kilos.					
2.15 P.M.	170	11·888	172	11·216	−0·672
4.15 "	163	11·188	220	13·648	+2·460
6.15 "	130	8·516	142	9·392	+0·876
Total ...	463	31·592	534	34·256	+2·664 = 8·433 p. c.

In this table it is seen that when 250 grams ($8\frac{1}{2}$ ounces) of sugar are added to a full meal the power of doing muscular work is increased.

In this instance it was the left hand that showed the greatest increase in working power, for it gave on the sugar day 16·599 per cent., while the right only yielded 8·433 per cent. more work. As usual, on both of the above days exactly the same quantities of food were taken, so that the only difference in them was the taking of 250 grams of sugar on the one and not on the other day. From this one is forced to conclude that sugar when added even to a copious meal has a most important power in increasing the human capabilities of doing muscular work.

I now give the result of the amount of work done on 250 grams of sugar in two periods of eight hours each, where in one case the sugar was taken in divided portions at three different times—100 grams ($3\frac{1}{2}$ ounces) at 8 A.M., and 100 grams again at 12 A.M., and 50 grams

($1\frac{3}{4}$ ounces) at 3.50 P.M.—the food partaken of, in all other respects, being on each day exactly the same.

Table showing total Gain in an eight-hour Day's Muscular Work produced by taking 250 grams ($8\frac{3}{4}$ ounces) of Sugar in addition to an Ordinary Diet.

Ordinary diet.			Ordinary diet + sugar.		
Time of day.	Time during weight lifted, seconds.	Total work, kilogram-metres.	Time during weight lifted, seconds.	Total work, kilogram-metres.	Gain in work, kilogram-metres.
Left middle finger raising a weight of 3 kilos.					
9.30 A.M.	108	5·691	114	6·741	+ 1·050
11.30 „	102	6·219	106	6·651	+ 0·432
1.30 P.M.	112	7·131	102	7·742	+ 0·611
3.30 „	138	5·073	126	6·966	+ 1·893
5.30 „	84	4·830	108	7·221	+ 2·391
Total...	544	28·944	556	35·321	+ 6·377 = 22·032 p. c.
Right middle finger lifting a weight of 4 kilos.					
9.40 A.M.	112	7·108	172	11·664	+ 4·556
11.40 „	108	7·512	150	10·360	+ 2·848
1.40 P.M.	146	8·420	148	10·392	+ 1·972
3.40 „	126	7·236	148	8·880	+ 1·644
5.40 „	104	7·752	158	10·368	+ 2·616
Total...	596	38·028	776	51·664	+ 31·636 = 35·858 p. c.

It will be here noticed that the gain in work during the nine hours by taking sugar was in the case of the left finger 22·032 per cent., and the right finger 35·858 per cent. And that fatigue was retarded in the one case 12 seconds and in the other 180 seconds.

Thus it is seen that when 250 grams ($8\frac{3}{4}$ ounces) of sugar is taken along with ordinary diet, it not only prolongs the time during which work can be done, but also increases the total amount of work performed in eight hours. It is further seen that the sugar taken at 3.50 P.M. had the effect of increasing the work done at 5.30 and 5.40 P.M., and that, instead of there being the usual diurnal fall in the amount of work done in the afternoon, there was actually an increase in the total amount of work accomplished.

Conclusion:—

1. Sugar when taken alone is a muscle food. 500 grams ($17\frac{1}{2}$ ounces) of sugar increased in my case the amount of muscular work done on a fasting day from 61 to 76 per cent.
2. The muscle energy-producing effect of sugar is so great that 200 grams (7 ounces) added to a small meal increased the total amount of work done from 6 to 30 per cent.
3. That when sugar was added to a large meal it increased the total amount of work done from 8 to 16 per cent.
4. That the work done during a period of eight hours can be increased from 22 to 36 per cent. by taking 250 grams ($8\frac{3}{4}$ ounces) of sugar.
5. That when sugar is taken at 3.50 P.M. it not only obliterates the normal diurnal fall in the muscular power, which usually occurs at 5.30 P.M., but even causes an actual increase in the total amount of work done.

VI. "Note on some Changes in the Blood of the general Circulation consequent upon certain Inflammations of an acute local character." By C. S. SHERRINGTON, M.D., F.R.S. Received December 11, 1893.

(Abstract.)

This note describes, and attempts to interpret, in a preliminary manner certain anatomical alterations in the blood noted in experiments on the acute local inflammation of various tissues. The contents of the communication are as follows:—

I. (1) Description of the method employed for the induction of the local inflammation.

(2) Description of the methods followed in examining the composition of the blood.

II. Summary of the alterations observed. The chief of these alterations are (1) inspissation of the blood; (2) reduction of the number of hæmic leucocytes followed by increase of the number of them, followed in some cases by a final decrease of their number to below the original normal; (3) disturbance of the mutual ratios normal between the different kinds of hæmic leucocytes. The summary is illustrated by quotation of protocols from certain of the experiments—

α. When the site of inflammation is in the limb.

β. When the site of inflammation is primarily peritoneal.

γ. When the site of inflammation is primarily in a mucous surface.

III. Interpretations offered for the hæmic changes found—

(1) For the inspissation observed in the blood.

(2) For the fall, and subsequent rise, of the number of leucocytes in the blood.

(3) For the disturbance of the ratios normal between the numbers of the different kinds of leucocytes in the blood. This last subsection is preceded by a brief account of the cell characters upon which reliance has been placed in the sorting of the hæmic leucocytes into the following groups:—

α. Finely granular leucocytes.
β. Coarsely granular leucocytes.
γ. Hyaline leucocytes.

VII. "On the Cœlomic Fluid of *Lumbricus terrestris*, in reference to a Protective Mechanism." By LIM BOON KENG, M.B. Communicated by Professor C. S. ROY, F.R.S. Received August 5, 1893.

[Publication deferred.]

The Society adjourned over the Christmas Recess to Thursday, January 18, 1894.

Presents, December 14, 1893.

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INDEX TO VOL. LIV.

ERRATUM.

P. 331, line 7, *for* "an asymmetrical" *read* "a symmetrical."

END OF FIFTY-FOURTH VOLUME.

HARRISON AND SONS, PRINTERS IN ORDINARY TO HER MAJESTY, ST. MARTIN'S LANE.

OBITUARY NOTICES OF FELLOWS DECEASED.

James Jago, B.A. (Cantab.) and M.D. (Oxon.), was a physician of considerable repute in West Cornwall. He was born on December 18, 1815, at the barton of Kigilliack, Budock, near Falmouth, once a seat of the Bishops of Exeter. He was the second son of Mr. John Jago, and the representative of an old Cornish family, who were resident in the parish of St. Erme, near Truro, before the year 1588. One of his lineal ancestors was a staunch Parliamentarian, who was appointed a Commissioner of Sequestration by Oliver Cromwell, after the death of Charles I. Young Jago received his early education at the Falmouth Classical and Mathematical School, where he remained a pupil until about 1833. About this time he expressed a strong desire to go through a course of training at one of the Universities, but preparatory to this he had the benefit of some private tuition. He had, however, always a great respect for the instruction he received in the Falmouth School, and he retained a deep interest in its prosperity to the end of his life.

In 1835 Mr. Jago entered St. John's College, Cambridge, as a pensioner, and graduated B.A. in the Mathematical Tripos of 1839 as 32nd Wrangler. Soon after obtaining his degree, he resolved to adopt the medical profession as his future occupation of life. For this purpose, and to obtain the necessary qualifications, he studied at various hospitals in London, Dublin, and Paris. But anxious to obtain a good medical degree, he resolved to go through a special second course of training at the University of Oxford, where he accordingly entered his name as a student, both in arts and medicine, on the books of Wadham College, from which he graduated B.A. and M.B. in 1843, and finally M.D. in 1859.

During the early years of his professional career, after he had chosen Truro for his residence, Dr. Jago was a voluminous writer on various medical subjects, the most important of which were investigations on certain special diseases of the eye. One of his first contributions on this subject, contained in a series of papers published in the 'London Medical Gazette,' is that entitled "Points in the Physiology and Diseases of the Eye." In these papers he developed certain entoptical methods of exploring the eye by means of divergent beams of light, which he considered to be an explanation which preceded all like solutions of the problem. In 1854 he communicated to the Royal Society a paper on "Ocular Spectres and

Structures as Mutual Exponents," which was followed by another on the same subject in 1856. In 1857 a paper "On the Functions of the Tympanum" was also read before the Royal Society. These three papers are published in the 'Proceedings.' Among his other medical papers, which are mostly inserted in the proceedings of kindred societies, or in medical journals, the following titles will give a good notion of Dr. Jago's original investigations. "The Eustachian Tube: why opened in Deglutition?" 1856; "Pains in the Abdominal and Thoracic Walls," 1861; "Ophthalmoscopic Muscæ Volitantes in a very Myopic Eye," 1861; "Medicine as influenced by Scientific Tendencies," 1861; "Entacoustics," 1868; and important papers on "Entoptics," published in the 'British and Foreign Chirurgical Review,' 1859. So much interest was taken in Dr. Jago's papers on Entoptics that he was encouraged to continue his investigations on this subject, which resulted in a publication of a separate treatise in 1864 under the title of 'Entoptics, with its Uses in Physiology and Medicine,' giving not only his own views in some detail, but also those of other writers. This work is an exposition of a difficult subject, especially as the author has ventured on untrodden ground while investigating and suggesting explanations of phenomena which had not hitherto been sufficiently accounted for. Dr. Jago was also a contributor to the 'Journal of the Royal Institution of Cornwall,' which contains his papers "On Observations of the Solar Phenomenon of April 5, 1871;" "Nangitha Cross," with illustrations; and "Ancient Cross at Trelanvean, St. Keverne."

During the last forty years or more, Dr. Jago took a considerable interest in the proceedings of the Royal Institution of Cornwall at Truro. He had served as Honorary Secretary for many years, and in 1873 was elected President for two years. His presidential addresses, delivered at the annual meetings of the Institution, have all been marked as giving the history and progress of Cornish science, and even at the present time may be read with profit. As a Vice-President he continued so long as he was able to take his personal share of work, and his presence on all occasions was always looked upon as certain. Lately, however, owing to his feeble health, he was compelled to retire altogether from any active participation in the management of the Institution. This forced retirement of Dr. Jago from so many of his old associations was much regretted by his friends. He, however, remained a Vice-President until his death, and, though he was unable to attend the meetings, his interest in the proceedings never abated. In 1856, Dr. Jago was appointed Physician to the Royal Cornwall Infirmary, and he was also connected with other medical institutions at Truro. On June 2, 1870, he was elected a Fellow of the Royal Society.

Dr. Jago was married in 1864 to Maria Jones, daughter of Mr.

Richard Pearce, of Penzance, by whom he leaves two daughters. Seven or eight years ago he had an attack of paralysis, which compelled him to retire from practice. He had since been an invalid, getting gradually weaker from year to year, though he was able to take his usual daily drive till within a few days of his death. He died on January 18, 1893, at the age of seventy-seven.

E. D.

The Rev. CHARLES PRITCHARD, D.D., Savilian Professor of Astronomy in the University of Oxford, was born at Alberbury, Shropshire, on February 29, 1808, being the fourth son of Mr. William Pritchard. In his early youth he was sent to a private school at Uxbridge, of which his recollections were slight, but the little that he did remember of its internal and economical arrangements were not pleasant. When nearly eleven years old he was removed to Merchant Taylors' School, to which, according to an entry in Robinson's Registers of the school, he was admitted in January, 1819. In those hardy days it was the custom of the school to commence work at 7 in the morning, and, as young Pritchard's home was at Brixton, he was accustomed, for a year and a half or more, to take this long and weary walk of nearly four miles at a very early hour, regardless of rain or fog. Referring to this period of his schooldays, in his 'Annals of our School Life,' Pritchard says: "I do not remember that I ever complained of this severe arrangement; I was old enough to be aware that temporary economical necessities were the cause, and I can never forget that the words most frequently heard in my home were 'education,' 'education'; so I suppose I felt I was being 'educated.'" Apparently, however, he did not profit so much as he desired from the instruction he received at this school.

From Merchant Taylors' young Pritchard was transferred to an admirable academy at Poplar, conducted by a Mr. John Stock—a self-educated, energetic, and practical man, of very considerable abilities. Here he felt more in his natural element, and he always referred to the practical training he received at this school with the greatest respect. In addition to the general school education usually obtainable at that time, the senior pupils were indulged with the sight and handling of a number of instruments and working models made and used by the celebrated Ferguson, the astronomer. These included some telescopes and quadrants, which excited the curiosity of young Pritchard and other intelligent youths, and probably, in his case, formed the turning-point of scientific proclivities. He has himself said that "very many of us could use the theodolite, and could survey and plot an estate. Our practice-ground was mainly in the Isle of Dogs, at that time an all but unoccupied waste, and I well remember how, at the age of less than sixteen, I earned two guineas

for indoctrinating an intending colonist in the art of field-surveying. I did not leave him until we had completed the plan of Kennington Common, and had calculated its acreage." On leaving this excellent private school, after about two years' tuition, young Pritchard had an opportunity of becoming a private pupil of the Head Master of Christ's Hospital School, who was at that time permitted to take a limited number of private pupils and place them in the public classes under his own personal care. Here young Pritchard attended to his classical studies with great diligence. He was accustomed to recall to his mind, with satisfaction, that, for about a twelvemonth, he was placed at the head of the Deputy Grecians.

Owing to family pecuniary difficulties, arising from the failure of a manufacture conscientiously but unsuccessfully persevered in by his father, young Pritchard was reluctantly withdrawn from Christ's Hospital School. A question now naturally arose in his family as to the future occupation of the youth, who was not yet seventeen years of age, and considered to possess a more than average amount of scholastic knowledge, scientific tastes, and literary abilities. Fortunately, by the advice and intervention of an elder brother, he was permitted to follow his own devices for continuing his education as best he could, with the ultimate hope that means would be found somehow to enable him to enter one of the Universities. "So," he has remarked, "I was left to my own resources, and happily a genuine love of knowledge of any and every sort stirred within my intellectual frame; and, inasmuch as the most attainable form of knowledge for the untutored was, and still is, mathematics, so to mathematics I betook myself with a will."

These two years, 1824—1826, were mostly devoted to self-instruction, and in this interval he made some acquaintance with the contents of Wood's 'Algebra,' Woodhouse's treatise on 'Plane and Spherical Trigonometry,' Dr. Lardner's treatises on 'Analytical Geometry and the Differential Calculus,' and other mathematical works. At the same time he attended some courses of lectures on chemistry, delivered at Guy's Hospital. He was much interested in these lectures, and ever after in the science, the benefit of which was reaped in after years at Clapham. In 1825, when only seventeen, he first felt the ambition of authorship, and published an 'Introduction to Arithmetic,' in which the elementary properties of numbers are explained and demonstrated on the simplest principles. In many respects, these two years turned out to be an important epoch in the life of Pritchard, for by the most determined perseverance in his own studies, and by the assistance of friends and relatives, who engaged to furnish temporarily the requisite funds, he was enabled, soon after Easter, 1826, to enrol his name on the books of St. John's College, Cambridge.

While in residence at Cambridge, Pritchard was fortunate in having the assistance of Charles Jeffreys as his private tutor—an excellent mathematician, and Second Wrangler in Airy's year, 1823. He, from the first, took a good position in the college examinations, and always secured the second place in each year. The practical result of this success was of the highest importance to him, as the accumulation of exhibitions was ultimately sufficient to defray all the necessary expenses of his college education. In the Mathematical Tripos of 1830, Pritchard attained the high position of Fourth Wrangler. He was himself fully satisfied with the result, though it was the general opinion of the college tutors who had watched his career that he had hardly done himself full justice in the examination. His position in the Tripos was, however, sufficient to secure for him a limited number of private pupils, without interfering with his classical studies, to which he now devoted most of his time, as he was unwilling to risk the Fellowship examination, then almost wholly classical. In March, 1832, he attained the height of his ambition, by being elected a Fellow of his college. Pupils now flocked to him in superabundance, and appearances seemed to indicate that he was destined to settle down as a resident Fellow, and take an active share in the public tuition of his college. But other circumstances soon arose, preventing any arrangement of this kind. Some scholastic employment having been offered to him in connexion with a new proprietary school in London, he determined to forego his University prospects, and to seek his fate in the larger world of the metropolis. Writing more than fifty years afterwards, on referring to this important crisis in his life, he remarked that "looking back now through the vista of half a century, I cannot wholly satisfy my mind as to all the motives which impelled me, at so early a period of a successful academical career, to relinquish the natural hopes and ambitions which must have legitimately presented themselves. It might have been impatience. But still, looking back through the busy occupations of many subsequent years, I am inclined to doubt if I could have occupied them more advantageously in any other *rôle* of life than that in which I have actually engaged."

When still an undergraduate, Pritchard's originality of thought often induced him to consider other mathematical questions than those required in the college examinations. He was especially interested with certain trigonometrical relations brought to light by the mathematician Poinsot, which led him to examine other writings of the same author, more particularly his treatise on the 'Theory of Statical Couples.' Pritchard became quite enamoured with the singular power and wide application of the theory, and also with the clear light that Poinsot had thrown on much that had hitherto been obscure in the theory of mechanics. He has said with enthusiasm:—

"I could not rest until I had simplified the subject and brought my joy and my light within the ken of others." His interest in Poinsot's work resulted in the publication of a little treatise on the 'Theory of Statical Couples,' which was sufficiently popular to run through two editions, and to be adopted in the general University teaching. About the same time he contributed to the Cambridge Philosophical Society a paper on "The Figure of the Earth," consisting of a simplification of the final propositions in the mathematical treatment of the Earth, considered as heterogeneous.

In 1833 Pritchard accepted the post of Head Master of a new proprietary school at Stockwell—one of those founded about that time in the suburbs of London under the auspices of King's College. The tenure of this office was, however, in no way an agreeable one, for, from the date of his appointment, his relations with a minority of the Committee were unfortunate. The school flourished numerically, notwithstanding the ceaseless interference and sundry small annoyances to which he was subjected. As time went on, these personal differences increased, until they became so unbearable to Pritchard that in June, 1834, he resigned his office, and had serious thoughts of returning once more to the more peaceful occupation of a University life. But, owing to the prospect of an early marriage, by which his Fellowship would be vacated, and finding that some of the leading men in Clapham, and also many of the parents of his pupils, had expressed a strong desire for the establishment of a new school under better and more liberal regulations, he, after some consideration, consented to superintend such an institution. On this basis the well-known Clapham Grammar School was founded in August, 1834.

In this school Pritchard continued to be the central and controlling spirit during the following twenty-eight years, labouring in the cause of high middle-class education with untiring energy and success, by which he obtained very rapidly a high reputation as a successful teacher. The many schemes he devised during this period for the thorough training of his numerous pupils have been highly appreciated. Some of them have been adopted with advantage in other schools. In an article contained in the 'Nineteenth Century' for March, 1884, the Dean of Westminster, Dr. Bradley, who received his early education at Stockwell, and afterwards at Clapham, bears personal testimony to the enlarged and generous views of his old teacher. The article gives a most interesting description of the early days of the school, and of the practical methods employed by Pritchard to interest the boys in their studies:—"He, first of all, at a time when the real study of comparative philology was almost unknown in England, gave us some glimpses into what I may call the science of language; he taught us to try to group together facts for ourselves, and to form laws from what we observed and met.

And he did more, he taught us something at the same time of the beauty and charm of literature, old and new. But this was not all; no single week passed in which we did not receive and eagerly look forward to at least one lesson in natural science. Heat, elementary hydrostatics, mechanics, optics, electricity, and, above all, chemistry—to something of the elements of all these we were introduced in turn. Meantime we were led through stage after stage of the severe discipline of mathematical study. I felt then, as I feel now, that even the study of mathematics was coloured with the warm glow of the activity and originality of the teacher's mind."

The reputation of the Clapham Grammar School was sufficient to attract pupils from all parts of the kingdom. One important feature was the interesting fact that among them might be found the sons of distinguished men at the head of the several branches of science, and of the liberal professions. The names of Airy, Barry, Darwin, Gassiot, Grove, Hamilton, Herschel, Maurice, and others, became familiar in the roll-call. For reasons possessing only a personal interest, Pritchard brought his Clapham life rather suddenly to a close in the year 1862, when he transferred all his interests in the school to Dr. Alfred Wrigley, one of the Professors at Addiscombe. He then retired, with his family, to Freshwater, Isle of Wight, where he hoped to enjoy a few years' repose, intending afterwards to apply himself to the permanent duties of some pastoral charge.

Pritchard was of far too active a mind to remain long in retirement. For some time his ambition seems to have been directed to some preferment in the Church, but his hopes were doomed to disappointment. He always considered himself to be a divine in mind and heart, though, by the force of circumstances, he became first a schoolmaster and then a professional astronomer. It has been stated by one of his late assistants that, "so anxious was he for a cure of souls, that he applied to one of his old pupils, who was then a Bishop, and asked for a living of only £100 a year. He was refused, and felt the refusal keenly." In some form or other he was, however, frequently engaged in clerical work during his seven or eight years of retreat. On several occasions he was invited to preach before the British Association at the annual meetings, first at Nottingham in 1866, and afterwards at Dundee in 1867, Norwich in 1868, Exeter in 1869, and Bristol in 1876. He also delivered addresses, generally on the harmony of the Bible and science, at various Church Congresses. Vice-Chancellor Page Wood, afterwards Lord Chancellor Hatherley, to whom Pritchard alludes as "the friend of his life," was much impressed with the treatment of the subject in the Nottingham sermon, and was induced to write a short treatise on the 'Continuity of the Holy Scriptures,' based on the same line of argument adopted by Pritchard. This celebrated sermon led to his appointment as Hulsean Lecturer

at Cambridge in 1867. He was one of the Select Preachers at Cambridge in 1869, and at Oxford in 1876 and 1877.

Pritchard's occupations during his residence at Freshwater were not by any means confined to clerical duty, as for some time before and after he left Clapham he felt much personal interest in the affairs of the Royal Astronomical Society, and in astronomical researches generally. The first paper contributed by him to the Society, bearing upon the practical part of astronomy, is contained in the 'Monthly Notices' for January 14, 1853, giving an account of some experiments towards increasing facility and certainty in the use of mercury in observations by reflexion, and for the adjustments of astronomical instruments. In 1856 he became a member of the Council, and shortly afterwards read a paper, the result of considerable calculation, on "The Conjunctions of the planets Jupiter and Saturn in the years B.C. 7, B.C. 66, and A.D. 54." This memoir was written to correct an astronomical error in which Ideler and others had fallen, while attempting to establish the date of the true *Annus Domini*. Astronomy, indeed, was not neglected at Clapham, for an observatory, furnished with an equatorial and a transit instrument, was actually added to the other institutions of the school, and Pritchard built another observatory for his private use at Freshwater. This long personal interest in astronomical research as an amateur led to his appointment, in 1862, to the responsible post of Honorary Secretary of the Royal Astronomical Society, and, subsequently, to that of President for two years, 1866—1868. His zeal for the interests of this Society and the promotion of astronomy was so great that, though resident in the Isle of Wight, he made it his duty to be present at most of its meetings. It is a pleasing record of scientific devotion to state that, during his two years' tenure of the Presidency, he was able to preside over the ordinary meetings fourteen out of a possible sixteen times. His addresses delivered at the anniversary meetings of the Society, on presenting the Gold Medal to the medallists of 1867 and 1868, are not only models of elegance of language, but they are also masterly expositions of both the new and old astronomy, in connexion with those sections of the science for which the Medals were respectively awarded: that of 1867 to Huggins and Miller for their joint researches in astronomical physics; and that of 1868 to the great French astronomer Le Verrier for his sublime mathematical investigations on the planetary theories, and the construction of new tables of the motions of Mercury, Venus, the Earth, and Mars in their orbits.

By the death of Professor Donkin in November, 1869, the Savilian Professorship of Astronomy at Oxford became vacant. Candidates from all parts of the world were eligible, and the appointment of a new Professor was at that time in the hands of thirteen trustees, in-

cluding Pritchard's friend, Lord Chancellor Hatherley. No one knew more than Sir John Herschel of the qualifications of Pritchard to fulfil the required conditions necessary for giving sound instruction on theoretical and practical astronomy. It was, therefore, by Sir John's urgent recommendation, together with the advice of other leading Fellows of the Royal Astronomical Society, that the Lord Chancellor was prevailed upon to exercise his great influence in his favour. No doubt this high patronage had its value, for at a meeting of the trustees, held early in 1870, he had the good fortune to be elected Savilian Professor of Astronomy.

Now comes a remarkable example of the intellectual strength and energy of Pritchard's character. At the date of his appointment he had reached the age of sixty-two, and, at the same time, had had very little personal experience of the practical work required in a large observatory. Most people of his age and habits, after having passed a busy life, are naturally looking forward to some relief from their ordinary daily occupations; or, at any rate, they are generally desirous to maintain, in any new official position, some conservatism of ideas and methods. But this was in no way the case with Professor Pritchard, who at once resolved that his Professorship was to be no sinecure for him; but that, on the contrary, he felt sure that some instrumental means would be found to enable him to contribute his share towards the progress of some of the most delicate problems in astronomical physics. To effect this, however, a new University Observatory would be required, and how this could be accomplished was for some time the principal subject that occupied his thoughts. In due time he laid his proposition before the governing authorities of the University, requesting that the Savilian Professor should be provided with astronomical instruments adequate to the instruction of his class and for the purposes of original research. The formal application was made to Convocation in March, 1873, when a liberal sum of money was granted, sufficient for the purchase of a refracting telescope of $12\frac{1}{4}$ inches aperture, and for erecting a suitable building to contain it. This grant was soon afterwards supplemented by Dr. Warren De La Rue's munificent gift of his 13-inch reflecting telescope, and many other valuable astronomical instruments, formerly belonging to his private observatory at Cranford. Thus, before the end of 1875, the University Observatory was completed, fully equipped and ready for active work for the promotion of the study of practical and philosophical astronomy.

It is not possible to find space in this notice for an adequate description of the numerous contributions to astronomical physics emanating from the labours of Professor Pritchard and his two assistants. With instruments of the most modern construction, they have been able to accomplish much that is new in some of the most

delicate branches of astronomical observation, without intrenching on the regular work of other official observatories. The results have been mostly printed in the 'Memoirs' and 'Monthly Notices of the Royal Astronomical Society,' or in the 'Proceedings of the Royal Society,' to which reference should be made for the details of the separate researches. Of the fifty papers chiefly contributed to these Societies since 1870, it will suffice, here, to give the titles only of a few of the most important: "On the Moon's Photographic Diameter, and on the Applicability of Celestial Photography to accurate Measurement"; "On a simple and practicable Method of measuring the Relative apparent Brightnesses or Magnitudes of the Stars with considerable accuracy"; "On certain Deviations from the Law of Apertures in relation to Stellar Photometry, and on the Applicability of a Glass Wedge to the Determination of the Magnitudes of Coloured Stars"; "On the Parallax of 61 Cygni, as obtained by the aid of Photography"; "Photometric Determination of the Relative Brightness of the Brighter Stars North of the Equator"; "Uranometria Nova Oxoniensis," containing the relative magnitudes of 2,784 stars, determined by the wedge-photometer; "On the Relative Proper Motions of 40 Stars in the Pleiades"; "On the Capacities, in respect of Light and Photographic Action, of two Silver on Glass Mirrors of different Focal Lengths"; "Determination of the Parallax of 30 Stars, chiefly of the Second Magnitude, by the Photographic Method," &c. Professor Pritchard also undertook a share of the observations for the new International Photographic Chart of the Heavens. The special zone of six degrees between 31° and 25° N. declination has been allotted to the Oxford University Observatory, and, at the time of his death, some progress in the work had been made.

In addition to the astronomical researches carried on under Professor Pritchard's direction, some most excellent papers and treatises of a popular nature were written by him from time to time. Not the least interesting are the three *éloges* contributed to the 'Annual Reports of the Royal Astronomical Society' for 1865, 1866, and 1872, on the deaths of F. G. W. Struve, Director of the Pulkowa Observatory, Sir W. Rowan Hamilton, and Sir John F. W. Herschel. He wrote a series of popular articles on astronomy for 'Good Words,' and was the author of "The Star of the Magi" in the 'Biblical Dictionary,' and of several articles in the ninth edition of the 'Encyclopædia Britannica.' He also collected some of his miscellaneous writings into a volume entitled 'Occasional Thoughts of an Astronomer on Nature and Revelation.'

His Savilian Lectures, both on theoretical and practical astronomy, were usually well attended, especially by intending candidates for mathematical honours. Owing to constant and increasing applica-

tions of University students for practical instruction, he provided a subsidiary observatory on the roof of the lecture room for their special use. Recently he erected a more convenient building, which he furnished with some excellent instruments. Every accommodation was thus provided for the instruction of the students, without in any way interfering with the larger instruments reserved solely for research.

Professor Pritchard proceeded to the degree of M.A. (Cantab.) in 1833, M.A. by decree (Oxon.) in 1870, and B.D. and D.D. in 1880. He was ordained Deacon in 1833, and Priest in 1834. On taking up his residence at Oxford, he attached himself to New College, of which, as Savilian Professor of Astronomy, he became a Fellow in 1883. He was elected, in 1886, an Honorary Fellow of St. John's College, Cambridge—an honour he greatly esteemed. He was a Fellow of the Royal Society for more than half a century, having been elected so long ago as February 6, 1840. He served on the Council two years, from November, 1885 to 1887, and at the Anniversary Meeting held on November 30, 1892, he was presented with one of the Royal Medals for his successful labours on photometry and stellar parallax. He was elected a Fellow of the Royal Astronomical Society on April 13, 1849, and was a continuous member of the Council from 1856 to 1877, and from 1883 to 1887. In 1886 he was awarded the Gold Medal of that Society for his 'Photometric Researches.' He was also a Fellow of the Geological Society and the Cambridge Philosophical Society. As Savilian Professor of Astronomy, he was an *ex-officio* member of the Board of Visitors of the Royal Observatory, Greenwich.

The great age to which Professor Pritchard attained never interfered with his determination to make the University Observatory a first class institution. His mental faculties were unclouded to the end; he was always able to keep abreast with the newest problems in the physics of astronomy, and it was a frequent and pleasing sight to witness the venerable astronomer enter into the depths of a technical discussion, with all the interest and energy of youth. In the midst of his scientific and University career, he did not, however, forget the busy time he had passed with his pupils in his old school at Clapham. In 1886, soon after he had received both scientific and college honours, it was a great joy to him to receive an invitation to a complimentary banquet at the Albion, Aldersgate Street, from his "Old Boys," among whom grisly beards and grey heads predominated. An interesting result of this social meeting of his old pupils, presided over by the Dean of Westminster, was a small volume, written by him for private distribution, full of pleasing reminiscences of his former school life.

Professor Pritchard was twice married:—(1) at Lambeth, on December 18, 1834, to Emily, fifth daughter of J. Newton, Esq.; and

(2) at St. Peter's, Croydon, on August 10, 1858, to Rosalind, daughter of Alexander Campbell, Esq., of Tunbridge Wells. His second wife predeceased him about a year. During the later years of his life, though naturally feeling the increasing weakness of old age, his health continued fairly good almost to the end, while the daily work of the Observatory never ceased to occupy his thoughts. He was fond of botany, and was a great lover of floriculture of the highest order. At one time he was supposed to possess one of the best collections of ferns in England. This love of flowers and plants continued as a pleasant recreation. It was only a week before he passed away that he was pointing out to a friend, with conscious pride, the beauty of the garden he had created around the Observatory. His death took place on the morning of Sunday, May 28, 1893, in the eighty-sixth year of his age; and on the following Wednesday afternoon his remains were laid to rest in Holywell Cemetery, Oxford.

E. D.

Henry Francis Blanford was born June 3, 1834, in Bouverie Street, Whitefriars, London, where his father, William Blanford, carried on a manufacture of gilt mouldings for decorative purposes, picture frames, &c., in premises now converted into the printing offices of the 'Daily News' newspaper.

The subject of the present memoir received his early education at schools in Brighton and Brussels, and after studying for some time at the School of Design, first in Somerset House, then in Marlborough House, he entered the Royal School of Mines, in Jermyn Street, at its commencement in 1851. At the School of Mines, he took the first place of the year, and at the conclusion of the first year's term, received the only prize then offered, the Duke of Cornwall's Scholarship. After leaving the school, he passed a year in studying mining at the Bergakademie of Freiberg, and another year, part of which he employed in translating v. Kobell's book on the blowpipe, his first published work, in London.

In 1855, Mr. Blanford and his brother, also a School of Mines student, received appointments on the staff of the Geological Survey of India, under the late Dr. T. Oldham, and they arrived in Calcutta at the end of September. Shortly after, the two brothers and Mr. W. Theobald, another member of the staff, were despatched to Orissa, to report upon a coal-field around Talchir, in the wild tract of the Tributary Mehals. Of this coal-field nothing except the existence of coal was known at the time; the whole of the geology had to be made out from the examination of the ground, the greater part of which was covered with forest. It was under these circumstances that, mainly through the observations of Mr. H. F. Blanford, the first steps were taken towards the classification of the remarkable series

of deposits associated with the Indian coal-bearing beds, by the separation of the underlying or Talchir division, and of an overlying group, from the true coal measures, subsequently called, by Dr. Oldham, the Damuda beds.

For some time Mr. H. F. Blanford was engaged in Calcutta, in charge of the Survey Office, and in palæontological work in the museum, but in 1857 he was placed at the head of a large survey party that was despatched to Madras, and he was chiefly engaged for the next three or four years in examining the cretaceous beds near Trichinopaly and Pondicherry, some fossils from which, described by Professor E. Forbes, Sir P. Egerton and Mons. A. d'Orbigny, had attracted much attention. The stratigraphy and the distinction of the different divisions in the field were founded on palæontological evidence, and the classification established by Mr. Blanford was fully confirmed by Dr. Stoliczka's subsequent exhaustive description of the fauna. A commencement of this description was made by Mr. Blanford himself, who, before he left the Indian Geological Survey in 1862, published an account of the Nautilidæ and Belemnitidæ in the 'Palæontologia Indica.' The geology of the area was described by him in the Memoirs of the Survey, to which he also contributed an account of the Nilgiri Hills.

Mr. Blanford's retirement from the survey was due to various causes, amongst which was the injury to his health produced by the exposure to the climate entailed by geological surveying. Soon after leaving India he was offered the Science Professorship in the Presidency College, Calcutta, by the late Mr. W. Atkinson, at that time Director of Public Instruction in Bengal. This appointment Mr. Blanford accepted, and after spending some months in Europe to recruit his health, he joined the staff of the Bengal Educational Department towards the end of 1862. He became, in 1864, one of the hon. secretaries of the Asiatic Society of Bengal, and about the same time, partly in consequence of his duties as secretary, his attention was directed to meteorology.* On October 5th, 1864, Calcutta was visited by one of the most destructive cyclones on record; a storm-wave rushed up the Hooghly River, and flooded the neighbouring low lands; upwards of 40,000 human beings were drowned, and a great part of the shipping in the river was wrecked. This cyclone was followed within a few weeks by another, which passed over Masulipatam, and the storm-wave again caused the loss of about 30,000 lives. These startling disasters naturally aroused the attention of the Indian Government and the public generally to the necessity of systematic meteorological observations, and to the im-

* The account of Mr. Blanford's meteorological work is by Mr. J. Eliot, his successor as Meteorological Reporter to the Government of India.

portance of establishing a proper system of storm-warnings for the protection of the ports of India, and especially Calcutta.

Up to the date mentioned almost the only trustworthy records of meteorological observations in India were those which had been kept for several years at the observatories of Madras and Bombay, and at the Surveyor-General's office in Calcutta. It is true that observations were also taken at a number of hospitals and dispensaries throughout India ; but the instruments had not been verified, the observers were untrained, and there was no proper supervision ; moreover no care had been taken to preserve the records. Mr. H. Piddington had collected and published in the 'Journal of the Asiatic Society of Bengal,' details of 23 different cyclones in the Indian and Chinese Seas, a work of the greatest interest and value, but the data, which were naturally imperfect, whilst adding greatly to the knowledge of these storms, and whilst sufficing to enable Mr. Piddington to frame practical directions for the guidance of sailors during such storms in the India seas, had not led to a full understanding of the disturbances, or of their origin.

At the instance of General (then Colonel) R. Strachey, who, in 1857, called the attention of the Asiatic Society to the uselessness of the desultory attempts that had up to that time been made to acquire a knowledge of Indian meteorology, and to the urgent need of some controlling authority capable of directing and utilising the work of observers in India, a committee was formed which, after some unsuccessful attempts at acting as a controlling power, drew up, in 1862, a report in which the establishment of a small centralized system by the Government was recommended. At the request of the Government, the Committee, after some delay, drew up a scheme for carrying out the system recommended. This was not submitted to the Government till 1865, after the occurrence of the Calcutta and Masulipatam cyclones. Meantime the Indian Government had been urged by the Lieutenant-Governor of Bengal to establish a system of storm warnings, and the Secretary of State, about the same time, recommended the record of meteorological data in connection with the suggestions and requirements of the Sanitary Commission. To the latter body the whole question of meteorological enquiry in India was referred, and in accordance with their recommendations, provincial meteorological systems were established in the Punjab and North-West Provinces in 1865, in Madras in 1866, and in Bengal in 1867. These systems were, however, quite independent of each other, and the opportunity of establishing a controlling authority, so emphatically urged by General Strachey and the Calcutta Committee, was postponed for several years.

An account of the Calcutta cyclone of 1864 was drawn up by Colonel Gastrell and Mr. Blanford, and was published by order of the

Lieutenant-Governor of Bengal in 1866. It gave a very full description of all the more important features of that cyclone, and, considering the unsatisfactory character of a large portion of the data, is remarkably complete. The conclusions based on the data and on the investigations of Piddington show a thoroughly clear grasp of the subject, and are, in almost every respect, in agreement with the results of later investigation on storm genesis and motion in India.

Soon after the Calcutta cyclone, at the instance of the Lieutenant-Governor, a Committee, of which Mr. Blanford was the secretary, was appointed to arrange a system of storm-warnings for the port of Calcutta. Observatories were established at a number of coast stations, and the observations made were telegraphed to Calcutta daily. The Bengal Provincial Meteorological Department was founded in 1867 for the combination of general meteorological observations with the continuation of this system of storm-warnings, and Mr. Blanford became Meteorological Reporter for Bengal, still retaining his Professorship, and lecturing, chiefly on chemistry and physics, in the Presidency College. The new Meteorological Department of Bengal at once took a very high position, and became known for the accuracy of its data and the thoroughness of its work, and the annual reports on the meteorology of Bengal, prepared by Mr. Blanford, increased in importance from year to year. He also, during the eight years that he held the post, published a series of meteorological papers in the 'Journal of the Asiatic Society of Bengal.' Of these the most important were "On certain protracted irregularities of Atmospheric Pressure in Bengal in relation to the Monsoon Rainfall" ('Jour. As. Soc. Bengal,' vol. 39, Part 2, p. 123); "On the Normal Rainfall of Bengal" (*t.c.*, p. 243); and "On some recent Evidence of the Variation of the Sun's Heat" ('J.A.S.B.,' vol. 44, Part 2, p. 21). The first of these papers directed attention to one of the more important features of Indian meteorology, and, probably, of tropical meteorology in general, the frequent persistency of abnormal variations of pressure over large areas in India for periods varying in length from a few months to two or three years, and the connexion between such prolonged abnormal features and large modifications in the distribution of rainfall. This was a subject which occupied much of Mr. Blanford's thoughts, and it is increasing in importance in connexion with the forecasting of the general character of the monsoon rains, now performed by the Indian Meteorological Department.

During the same period he contributed two papers, one "On the Origin of a Cyclone" ('Proc. Roy. Soc.,' vol. 17, 1869, p. 472), the other on "The Winds of Northern India" ('Phil. Trans.,' vol. 166, p. 563), to the Royal Society. In the second paper he utilised the data collected by the Meteorological Departments of Bengal, the North-Western Provinces, and the Punjab, in order to describe the

chief features of the normal air-currents over Northern India, and to trace out their origin and causes, so far as they could be discovered, in the local physical changes of the atmosphere. The broad features were skilfully worked out, and the relations of the north-east and south-west monsoon currents to each other, and to the temperature and other conditions of India, clearly shown. This paper was particularly interesting. Not only was it the first attempt to discuss this important question by the aid of fairly accurate data, but it was also the first essay on Indian meteorology as a whole, and the subject was for the first time treated as a problem of dynamic meteorology, and recent extensions of knowledge in the physical sciences utilised in the discussion of the various problems.

Shortly after the publication of this important paper the Government of India came to the conclusion that the provincial system without a central controlling authority was unsatisfactory and ineffective, and it was determined to constitute a centralised department on the lines laid down as essential by General Strachey some years previously. Mr. Blanford was, in 1874, in consequence of this change of system, transferred from the Educational Staff of Bengal, and appointed head of the new department. He was called upon, at the time of his appointment, to prepare a scheme for the reorganisation of the provincial meteorological systems and their consolidation into an imperial system, with himself, the Meteorological Reporter to the Government of India, as central controlling authority.

In the scheme drawn up, Mr. Blanford sketched first the general principles on which meteorological work should be carried out in India, and also made proposals for the extension of the work of observation and for the centralisation of the Department, in order to secure uniformity of methods and tabulation of the results. He also proposed the commencement of special series of observations to throw light on the regular diurnal and annual meteorological changes in India. The scheme was approved in its entirety, and brought into operation in the year 1875. It has stood the test of time thoroughly, and the Department has developed during the eighteen years of its existence in the directions fully anticipated by Mr. Blanford.

One of the first important labours of the Meteorological Reporter to the Government of India was to write the 'Indian Meteorologist's Vade Mecum.' This was primarily intended to inform the observers at Indian observatories what and how they were to observe, in order that their observations might be accurate and useful. This portion of the work forms Part I of the 'Vade Mecum.' In order to arouse an intelligent interest in their work, Mr. Blanford, in Part II, gave an interesting account of all the more important features of Indian meteorology so far as then known, together with explanations based on the data and ideas of recent advances in physical science. The 'Vade Mecum' at

once became a useful book of reference for Indian observers, and was practically the first treatise which placed the ideas of the modern school of dynamical meteorology in an easily accessible form before ordinary readers. It was for many years the best treatise on modern meteorology, and was the forerunner of the numerous treatises on the science which have been published in the United States, Germany, and England.

Mr. Blanford was Meteorological Reporter to the Government of India from 1874 to 1889, but during the last two years he was on furlough. During this period he wrote a number of short and valuable papers for the Asiatic Society's Journal, of which the following were the most important: "On the High Atmospheric Pressure of 1876–78 in Asia and Australia, in relation to the Sun Spot Cycle" ('Jour. As. Soc. Bengal,' vol. 49, Part 2, p. 70; 1880); "On the Relations of Cloud and Rainfall to Temperature in India, and on the opposite Variations of Density in the Higher and Lower Atmospheric Strata" ('J.A.S.B.,' vol. 50, Part 2, p. 69; 1881); "The Theory of the Winter Rains of Northern India" ('J.A.S.B.,' vol. 53, Part 2, p. 1; 1884), and a series of papers on the "Diurnal Oscillation of the Barometer."

During the same period he wrote a very important short paper for the Royal Society "On the Connection of the Himalaya Snowfall with Dry Winds and Seasons of Drought in India" ('Roy. Soc. Proc.,' vol. 37, 1884, p. 3). In this he dealt with a remarkable feature of Indian meteorology, viz., the effect of abnormally heavy snowfall in the Himalayan area in modifying the pressure and temperature conditions over Northern India during the hot weather, and hence the distribution of rainfall during the following south-west monsoon. He was the first to realise fully the importance in Indian meteorology of this factor which has become the basis for the seasonal forecasts now issued by the Indian Meteorological Department.

His most important work at this time was undoubtedly the series of annual reports on the meteorology of India (from 1876 to 1885) that he wrote, and the papers he contributed to the 'Indian Meteorological Memoirs,' which publication he initiated shortly after the establishment of the Indian Meteorological Department.

The subjects of these papers show how largely his mind was occupied with the regular diurnal and annual meteorological changes in India. He considered a full knowledge of these matters of primary importance in the present stage of our knowledge, and that their solution would throw valuable light on some of the most important abnormal features of Indian meteorology and might furnish a key for the explanation of these features.

His last and most important work of investigation was the monograph on "The Rainfall of India" ('Indian Meteorological Memoirs,'

vol. 3). It was the outcome of the labour of several years. All the available data were obtained and sifted to separate the doubtful from the trustworthy. The result of this investigation was to give an accurate knowledge of all the broader features of the distribution of the rainfall of India, and of the chief causes or factors (physical and topographical) determining the law of its distribution.

After he retired on pension in 1889 he continued to devote himself with unwearied zeal amidst failing health to the discussion of his favourite meteorological problems. He undertook the discussion of the series of hourly observations taken at about twenty-five stations in India from 1876 to 1888. He completed the discussion of those taken at Sibsagar, Dhubri, Goalpara, Hazaribagh, Patna, Roorkee, and Allahabad, but was obliged to give up the work in the beginning of 1892. It was his intention to have prepared separate statements and brief discussions of the results for each station, and to have followed this up with a general discussion of the whole of the results, and it is greatly to be regretted in the interests of meteorological science that he was not spared to complete this work on a subject to which he had devoted especial attention, and which he was especially qualified to investigate.

He presented the chief results of his investigations and those of his co-workers in India to the English public in 1889, shortly before his death, in his 'Climates and Weather of India.' It is a valuable work of local climatology, and presents all the more important results of the work of the Meteorological Department during his *régime* in an interesting form for English readers.

It will thus be seen that his life was one of unwearied activity. His powers of organisation were shown by the steady development of the department which he established and initiated. He was a patient and vigorous worker, and the results of his labours are shown as much by the numerous short suggestive papers he contributed to various Societies, &c., as by his larger monograph 'On the Rainfall of India,' and the Annual Reports on the Meteorology of India. His name will be associated with the commencement and development of scientific meteorology in India, and the rapid growth of the department under him is the best proof of his special qualifications as a meteorologist and of his zeal and untiring energy. European meteorologists recognised almost from the first the value of the work done by the department under him; it was his constant aim to place his department upon as high a level for scientific and practical work in meteorology as similar departments in Europe and America, and it is hardly too much to say that he fully succeeded.

After he became engaged in the work of Indian meteorology, Mr. Blanford's time was almost entirely occupied with that subject, although he by no means lost his interest in geology and zoology.

His only important contribution to Indian geology after he left the Indian Survey, was a paper published by the Geological Society in 1875, 'On the Age and Correlation of the Plant-bearing Series of India and the former existence of an Indo-Oceanic Continent.' He also contributed a few short papers to Indian scientific societies on land and fresh-water mollusca and on ferns. He was the author of two treatises on the geography of India, one of which has now for many years been used as a text-book in Indian schools and colleges, and the other, a recent publication, forms one of Macmillan's geographical series. Mr. Blanford married, in 1867, the daughter of Mr. G. F. Cockburn, of the Bengal Civil Service, and leaves a widow, one son, an officer in the Royal Artillery, and three daughters. He was elected a Fellow of the Royal Society in 1880, and was President of the Asiatic Society of Bengal in 1884–85. His health had been precarious for some time before he retired from the Indian Service, and he died of cancer at Folkestone, where he had resided after his return to England, on the 23rd January of the present year. W. T. B.

Dr. WILLIAM CHARLES HENRY was born in Manchester, March 31, 1804. His father and grandfather were both Fellows of this Society, and both distinguished chemists. He was educated at various schools, and matriculated at Edinburgh University in November, 1824. In 1827 he graduated M.D., the subject of his graduation thesis being "De Tuberculorum Origine," and in the following winter studied in the Paris hospitals, attending as well the lectures at the Sorbonne. From 1828 to 1835 he was physician to the Manchester Royal Infirmary, but resigned this post in order to continue his chemical studies. He studied at Berlin and Giessen, and afterwards returned to Manchester. Leaving Manchester about 1842, he took up his residence at Ledbury and remained there until his death on January 7, 1892.

Dr. Henry was elected a Fellow of the Royal Society in 1834; he was also a Fellow of the Chemical and Geological Societies, and a Corresponding Member of the Royal Academy of Sciences at Turin. He was the author of papers "On the Relation existing between Nerve and Muscle" ('Roy. Soc. Proc.,' 1831, p. 64), "On the Physiology of the Nervous System" ('Brit. Assoc. Rep.,' 1833), "On the Atomic Constitution of Elastic Fluids" ('Phil. Mag.,' 1834), "On the Action of Metals in determining Gaseous Combination" ('Phil. Mag.,' 1835), "On Gaseous Interference" ('Brit. Assoc. Rep.,' 1836), and was the author of "Memoirs of the Life and Scientific Researches of John Dalton" (Cavendish Society, 1854), Dalton having been one of his most intimate friends. M. F.